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Preface

From the winter of 1989 to the spring of 1991, the Journal published a series of essays on the social sciences in agriculture, particularly agricultural economics. Some essayists lauded the performance of the profession, others criticized. All presented viewpoints subject to challenge. We decided to pull the series together under a single cover and explain its significance.

The argument for self examination, if for no other reason, should be made on the grounds of fairness. If agricultural economists can examine the efficiency and effectiveness of the food system, why not ask about the efficiency and effectiveness of agricultural economics? We understand the suboptimal behavior of farmers, traders, bankers, supermarket managers, and pesticide applicators, so why not economists? Why not journal authors, editors, and publishers?

No one, least of all the authors and editors of these papers, would claim the final word on any of the topics addressed in this series. We do hope that here and there are thoughts that will stir you to reflect on what you are for your clients, colleagues, and cooperators.

Gene Wunderlich
Jim Carlin
Editors, JAER

THE JAER ESSAYS ON PROFESSION AND PERFORMANCE

Gene Wunderlich

Ten Perspectives

The Journal essays reprinted here provide 10 perspectives about purposes, mechanisms, and successes of agricultural economics. These perspectives are convenient entries into self examination and appraisal. They cause us to ask: What do agricultural economists do? What and how much do they produce? Are they efficient? By what standards are agricultural economists, or any social scientists, to be judged? The essays do not answer all these questions directly, but they provide some valuable insights. Some of the papers place agricultural economics in a larger frame of reference. Some of the papers enlarge a single technique or instrument of the profession. All of the papers contain useful ideas and viewpoints.

Doing Agricultural Economics

Clark Edwards partitioned the doing of agricultural economics into describing, explaining, and prescribing. Few would dispute the describing or explaining; the controversy lies in prescribing. The argument against prescribing rests on the limits of analysis. The argument for prescribing depends more on the purpose of problem solving and the need for relevance. Much of social science feels a tension between precision/elegance and usefulness/importance. Edwards does not relieve that tension, indeed, he encourages it. He argues for including prescription in the responsibilities of agricultural economists. His position would support the addition of philosophy and ethics to the training and experience of agricultural economists.

The life and times of Karl Fox is a classic case for broad, varied training and experience. His personal story is a metaphor for a substantial portion of the profession. For Fox, doing agricultural economics meant continuing education, writing in theory, and analyzing a wide spectrum of applied economic problems. (Fox's essay appears in draft form as the last entry in this publication.)

If prescription is to be an agricultural economist's responsibility, then more effort should go into integrating institutions and historical precedents into policy analysis, according to Otto Doering. Doering convincingly demonstrates that agricultural economics is deficient in relating to other social sciences. As agricultural economics becomes narrower and sharper, it becomes increasingly uncomfortable as a perch for policy analysis.

The Ingredients

Theory and facts comprise the essential ingredients of economic analysis. The stating and positioning of these ingredients determine the success of individual projects, and eventually the success of the discipline. Emery Castle, emphasizing the importance of theory, appeals for greater eclecticism and plurality of research approaches. Castle supports more introspection about the content and use of theory to explain or predict. Among his practical suggestions for research is a more complete reporting of failures and anomalies.

James Bonnen, meanwhile, attributes the credibility of agricultural economics to its solid basis in facts. He complains that the discipline emphasizes theory and elegance in econometric method, at the expense of its reputation as an empirical science. He stresses not only the respect for data but the regard for subject matter and problem-solving research.

To underscore the "can do" dimension of research, Tom Hertel presents an essay in the form of a working paper on computable general equilibrium models. He sets out to show that general equilibrium models are not necessarily any more complicated than some partial equilibrium models. Perhaps most important among the CGE virtues is a general perspective of an economy that enables the analyst to judge relevance and importance of sectors and influences.

Some ingredients of successful economic research must be drawn from beyond economics. Paul Thompson, astride philosophy and agricultural economics, insists that successful policy analysis draw from both (and other) disciplines. Thompson attributes, for example, the narrow, pecuniary coloration of agricultural economics research results to the consequentialist philosophy of economists. His solution to the integration of disciplines is working together on real world problems.

The Assessment

No simple positive or negative appraisal of the agricultural economics profession emerges from our series of essays. James Hildreth and Peter Barry are generally satisfied with agricultural economics and one of its instruments, the journals. Castle, Doering, and Daniel Hausman/Michael McPherson are less sanguine about how the profession addresses and resolves research issues.

Hildreth finds that agricultural economics serves policy, that the agricultural establishment recognizes economics, and that economists are doing their jobs. Perhaps, however, they must do better to offset the political and social forces presented by the organizations for which they work. He acknowledges the lack of

clearly defined markets for the type of information provided by economists.

Hausman and McPherson, drawing support from Donald McCloskey, assert that economists do not practice what their methodology preaches. Perhaps Hausman and McPherson do not distinguish when economists serve their profession, and when they serve a larger public. The two philosophers suggest that agricultural economists are more likely to be confusing on abstractions and philosophies than on practical problems.

From his experience as a journal editor, Peter Barry concludes that all is quite well in the world of intraprofessional communication. He referred to the journals as "gatekeepers" of scientific integrity, and the principal indicators of professional performance. He stressed the importance of journal stewardship because, as the indicator of professional performance, it is an instrument of professional reward.

The Journal as Tool of the Profession

The professional or technical journal serves the profession, and thereby indirectly, the public. The issue, then, is: Does its technical journal(s) adequately represent the contribution of a profession to the larger public it serves?

The affirmative, it seems to me, is that knowledge generation requires investment in discovering, refining, and testing ideas and propositions. Edwards called it describing and explaining. The journal is an instrument for saving and investing in knowledge. As banks enable, but do not create, real capital, technical journals enable new knowledge, but do not put it to use.

The negative is that journals become self-serving instruments of professional recognition. If volume of entries in the literature becomes "the name of the game," then replication, refinement, and review displace innovation and creativity. If journals are perceived, as Barry says, as gatekeepers of quality, potential contributors will reduce risk by confining their submissions to the familiar and acceptable. Journals become conservators of established principles and propositions.

If, as some evidence suggests, volume of entries in the technical literature has tended toward overproduction, perhaps a what-is-good-for-the-goose solution, at least for agricultural economics, would be a production control program, complete with base pages, publication allotments, and ARP's (authorship reduction plans). The program could be supplemented with a CRP (creativity restoration program). Only kidding, of course.

My purpose here is merely to state, and neither to debate nor resolve, the issue of the proper role of a technical journal in

public service. Clearly there is some role of technical journals in advancing knowledge. How they are used will depend on the attitude of the profession it serves. Most professions exhibit a degree of narcissism, and agricultural economics has registered its share of self-examination, adulation, and criticism. From the mammoth works of H.C. Taylor and AAEA review editors to the recent incisive articles by Randall, Just and Rausser, and McCloskey can be drawn a wealth of introspective knowledge and insight. About journals, particularly, the reader is invited to see Barry in this set of reprints, and in the readings following, Salant, Brorsen, and, for fun, Sykes.

Recommended Readings

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Publishing in Professional Journals

Peter J. Barry

My assignments as a past editor of the *Western Journal of Agricultural Economics* and as current editor of the *American Journal of Agricultural Economics* have provided unique opportunities to view the publication and research activities in agricultural economics. As an editor, I could not help but become better acquainted with the diversity of the subject matter in our field, the issues addressed, and the people involved. One gains a profound appreciation for the quest for knowledge and the intellectual efforts of people, working individually or collectively, to add to this knowledge.

As in most endeavors, the people involved in the journal process, authors, reviewers, journal readers, editors, are strongly motivated by self-interest. Some observers have recently worried that journal publication reflects too much the self-interest of those involved, especially authors, and that professional interests have become secondary, that creativity and risk-taking in journal publication are stifled, and that gamesmanship by authors (some reviewers, too) has become too prominent. My impression is that such concerns are exaggerated but are nevertheless features of the journal environment.

Professional journals provide several key functions, according to a recent article by Lacy and Busch.[1] First, journals disseminate information about new ideas, methods, institutions, theories, data, or ways of approaching problem situations. They foster scientific inquiry, dialogue, and debate and become the primary means of advancing an area of knowledge.

Journals are "gatekeepers". They serve a quality control function by vouching for the scientific integrity of the work involved. The decision to publish based on formal reviews of manuscripts by experts and editorial staff is a vital part of this function.

Lacy and Busch write further that journals have been responsible for enforcing scientific norms in the creation of disciplinary knowledge. That means they exercise a fair and consistent application of "objective standards in a universalistic manner, organized skepticism, disinterestedness, communality, and emotional neutrality." Editors and reviewers do not legislate the normative criteria in their respective fields. Rather, they are entrusted to apply the accepted and commonly understood research values of their particular discipline.

They conclude that journal publication is a forum that confers professional recognition and other rewards, because the performance of scientists is largely judged by their publications. Publication plays a major role in a professional's career advancement. The criteria often are imperfect because administrators and other evaluators may place more emphasis on the number of journal publications and where they are published than on the value of the contribution to the field. This places a still greater burden on the journals to evaluate the value of the authors' contributions.

How have journal functions evolved in a changing intellectual environment? Scientists know that keeping up with new developments in their field is especially challenging. Specialty areas become more refined and fragmented and are subject to periodic changes. Methods of analysis become increasingly sophisticated. Mathematical techniques often appear to dominate and new ideas may sometimes appear to be based more on refinements and twists, or on tinkering with existing models and methods rather than on resolving current problems or understanding key economic relationships. The depth, scope, and complexity of agricultural economics have expanded considerably, and the competition among scientists to produce rather than consume new knowledge has grown as well.

At the same time, professional journals have taken on greater importance relative to bulletins, reports, and other types of publications in reporting new scientific knowledge. Journals, therefore, have assumed more responsibility in verifying the integrity of the work

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[1]W.B. Lacy and L. Busch, "Guardians of Science: Journals and Journal Editors in the Agricultural Sciences," *Rural Sociology*, Vol. 47, 1982, pp. 429-48.

and testifying to the productivity of individual scientists. In response, journal submissions have risen as has the number of journals. And, even within a discipline, journals vie for professional status.

These developments have created a more complex environment for each of the major participants in journal publication. Small wonder that authors have become more strategic in planning publication activities, considering the growth in analytical sophistication, more publication outlets, greater competition, high degrees of uncertainty about publication prospects, the typically lengthy process of journal publication, and the professional rewards at stake. Keeping one's publication pipeline full, diversifying publication outlets, planning the sequence of submissions, and engaging in more joint work are examples of the elements of a publication strategy.

Journal editors, staffs, and reviewers must scrutinize contributions closely, and journal readers must be more strongly equipped to understand, evaluate, and synthesize published work of varying degrees of technical sophistication. Many readers will not read a general journal from cover to cover. Rather, the tendency of scientists to specialize in subject matter areas and analytical techniques, and the availability of other journals, suggest that individual readers will be attracted to few articles in a single issue. Finally, more effort is needed by the scientific community to move the knowledge reported in journals into education, policy, and managerial channels for the benefit of various clientele groups.

My perception of journal publication is that the process works well. I do not sense that authors have been inhibited from taking risks, have had creativity stifled by concerns about career advancement, or have engaged in manuscript reviews with an eye on their own work. (It is the editor's job, of course, to manage the review process to avoid such conflicts of interest between authors and reviewers.)

I do not sense that authors engage in excessive gamesmanship by submitting the same article to several journals, by misleading editors about prior publication, by trying to guide the review process, or by arguing about editorial decisions. (I do, however, have a few interesting exceptions filed away.)

I do not feel that authors exploit the journal's review process to improve the quality of their own work. Rather, it is natural to expect that reviewers' comments and suggestions will contribute to the quality of research and the effectiveness of its presentation. In my own work, the assistance from anonymous reviewers has consistently proved helpful, and I view manuscript reviews as an integral part of the knowledge-creating process.

Nor do I believe that the journal publication process is prone to a high incidence of error in the accuracy and validity of published work. Nonetheless, the periodic publication of comments and replies and observations of authors' occasional self-discovery of errors during the publication process indicates that this is a matter of concern. This topic received considerable attention in articles in the *American Economic Review, Science,* and other media in which serious questions were raised about difficulties in replicating published work, high incidences of error, and the integrity of some authors.[2] Some journals have gone so far as to require authors to submit their data, computer programs, and statistical routines along with their manuscripts. These requirements vouch for the accuracy of the work and encourage greater self-scrutiny by the authors themselves. Clearly, these issues will continue to command attention.

All participants in agricultural economics publishing must continue to make the journal process function as effectively as possible. This effectiveness will grow from placing the proper functions of journals ahead of personal gain. By focusing on enhancing knowledge creation, through the collective efforts of individual scientists, we will continue to advance our field, serve our clientele, and, in the process, benefit ourselves.

[2]For example, see W.G. Dewald, J.G. Thursby, and R. G. Anderson, "Replication in Empirical Economics: The Journal of Money, Credit and Banking Project," *American Economic Review*, Vol. 76, 1986, pp. 587-603.

A Critique of the World Agricultural Economics Research Establishment

R.J. Hildreth

At times, a good way to look at a profession is to step back, strip away the complexities, and simply ask: How are we doing? The answer for agricultural economics: Our work matters but innovation and flexibility will make us better.

My assessments of the state of the profession are:

- Agricultural economics research results increasingly affect policy debate and dialogue.
- The agricultural research establishment is expanding its use of agricultural economics.
- Agricultural economists are doing their jobs well.
- Agricultural economics research needs to balance emphasis among problem solving, subject matter, and disciplinary analysis.

Research Affects Policy

How its analysis is used is an important measure of a profession's effectiveness. There appears to be growing demand for agricultural economics research by public and private officials for use in public policy debates. Economic analyses are gaining acceptance by government officials, especially in many developing countries. For example, the drought and other food concerns in many African countries have expanded the use of agricultural economics information in policy decisions in these countries. Administration officials, legislators, and other authorities in the United States, Europe, and other countries are strengthening their reliance on agricultural economics research analysis.

Improved methods of analysis are perceived as useful and are used more quickly now in policy debates. For example, the time between the development of the concept of producer and consumer subsidy equivalents and their use in General Agreement on Tariffs and Trade (GATT) negotiations was comparatively short. The use of risk analysis techniques has expanded as the problems facing individual firm managers and government become more complex because of the internationalization of agriculture and increased uncertainty.

A significant measure of the quality of agricultural economics' analytic services can be inferred from the quality of the public debate on food and agricultural policy issues. A democracy's quality of public debate is a better measure of the usefulness of agricultural economic analysis than the "rightness" of policy decision. World agricultural policy has moved from debates that have included myth and dogma to a more realistic approach to problems. Examples include the current GATT negotiations and the U.S.-Canada free-trade dialogue about structural change in both agricultural organization and incentives in most socialist countries. Agricultural economic analysis has also made significant contributions to the quality of the debate within and between international organizations like the Food and Agriculture Organization of the United Nations (FAO), World Bank, and Organization for Economic Cooperation and Development (OECD).

Research Expands Role

Results of agricultural research have recently gained wider acceptance. The rising influence of international research centers, the growing concern about profitability as opposed to maximum yield per acre, and the broad-ranging implications of the internationalization of agriculture in all countries have led users in the agricultural industry to value agricultural economics research more highly.

Ironically, the profession of agricultural economics was developed in many countries mainly by agronomists and other biological scientists. They turned to economic analysis in an attempt to deal with the problems and opportunities faced by decisionmakers in agriculture. Advancements in theory, funding, and structure of agricultural research widened the gap between agricultural economics and other agricultural research fields. Many of the farm firm-oriented economics researchers worked closely with their biological science counterparts. The application of the pure

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theory of production economics to farm production processes by Earl Heady, his colleagues, and students is a model linkage between the two fields. Expansion in the use of computer-assisted decision aids by farmers has led to more joint efforts.

The recent farm financial crisis in many countries, especially the United States, has led to closer cooperation between economists and other agricultural researchers. And, a growing concern about the impact of farming practices on the environment has brought a focus on low-input, sustainable agriculture and profitability. For example, much of the rapid adoption of minimum tillage with high energy prices can be explained by lower cost and soil conservation.

Most of the international research centers recognize the need for agricultural economics research. Some of the efforts join economics and biological scientists, and some are economic research on policy and institutions that affect adoption of new genetic material and production practices developed by the centers.

Expanding is the number of agricultural economists appointed to research administrator roles in international centers, agricultural research organizations, agricultural experiment stations, universities, and government agencies. The leadership and perspective of these individuals have added to acceptance of agricultural economics by the agricultural research establishment.

Agricultural Economists Do Their Jobs Well

"The typical product of social science research is information," according to Bob Lindner in his 1987 presidential address to the Australian Agricultural Economics Society (*5,* p. 96).[1] Placing a value on the information produced by the world agricultural economics research establishment is a way to judge how well agricultural economists do their jobs. If an easily observable market for information existed, a demand curve for information could be estimated and shifts in the demand and supply curves identified. Although a demand for the information produced by agricultural economists exists, it is not easily observable, especially on a worldwide basis, and that leaves personal observation and interpretation.

The demand for information and the value of information is reflected in the perceived usefulness of the services of agricultural economists. The demand for services of agricultural economists includes a demand for services other than that of researchers. Economists educate undergraduate and graduate students as well as firm managers and citizens through extension services.

The price (salaries) of agricultural economists is a function of both the supply and demand. The salaries of agricultural economists appear to be climbing. The inference: agricultural economists are doing their jobs well.

Most agricultural economists are employed by public bodies, such as government agencies, universities, and international organizations. Both government agency and university employment jobs appear to have leveled off or declined. Some universities in the United States and United Kingdom are not filling vacancies. Reduced spending by governments, budget deficits, and, in the case of universities, slower growth in numbers of students due to demographic factors have contributed to the stagnation. However, the share of employment and budget for agricultural economics compared with other professions in agricultural research appears to have increased somewhat.

The employment patterns of private firms have changed greatly in the past decade. Middle management numbers have declined significantly as firms responded to market conditions and lower profits. The proportion of the membership of the International Association of Agricultural Economists (IAAE) from private firms appears to have slipped over the past 10 years. Many firms have eliminated their economic research departments with finance or marketing units often taking over economic analysis.

I conclude that while agricultural economists are doing their jobs well, they will have to do their jobs better to offset the political, social, and economic forces that affect the organizations for which they work.

What Agricultural Economists Do

Glenn Johnson examined the roles of agricultural economists at the 1976 International Conference of Agricultural Economists (*4*). Johnson discussed the contribution of agricultural economists in three significant roles: participants in decisionmaking, doers of subject matter analysis, and doers of disciplinary analysis.

Participants in decisionmaking focus on a particular problem, either public or private, and they merge theory, empirical knowledge, and command over qualitative techniques to develop empirical information into public or private prescriptions. Doers of subject matter analysis develop and gather information on a specific subject that is relevant for solving a set of problems. Doers of disciplinary analysis improve theories, qualitative techniques, and data.

[1]Italicized numbers in parentheses cite sources listed in the References section at the end of this article.

Ken Hunt, Oxford University, assessed changes in the thrust of agricultural economics over time, beginning with the 1920's in Great Britain (*3*). According to Hunt, the principal aim of agricultural economists in the 1920's and early 1930's was supplying management advice to farmers. Many of these economists came from the biological science side of agriculture. Hunt wrote that the increased professionalism among agricultural economists has encouraged segmentation of the subject and created an interest in the academic aspects of the profession, an increase in the pursuit of knowledge and not in application. Hunt saw that the concerns of agricultural economists have become broader and more diverse. Most agricultural economists specialize but still claim to be agricultural economists.

Castle and I have argued that the range of problems needing agricultural economic analysis will continue to expand, but a lack of agreement exists on how these problems should be investigated. We said that some agricultural economists and some academic departments of agricultural economics will become more pragmatic and interdisciplinary, while others believe that greater disciplinary depth will yield better returns over time (*1*, p. 12).

Debate about the thrust and role of agricultural economics has existed since the beginning of the profession. Taylor and Taylor reported the cleavage between rural economics and farm management in the early 1900's (*7*). The policy statement of the new international journal *Agricultural Economics* lists three areas of coverage: disciplinary topics, subject matter topics, and problem-solving topics. Hedley (*2*, p. v) commented: "This last area of problem solving is a particularly difficult one from which to obtain well-documented research and endeavor since many professionals involved in this work, even though they may have considerable training in agricultural economics, have little encouragement to publish."

A Survey of Agricultural Economics Literature, edited by Martin and sponsored by the American Agricultural Economics Association (AAEA), showed the changes in emphasis in roles over time (*6*). Reviews of agricultural economics literature published in the Australian *Review of Marketing and Agricultural Economics* and the British *Journal of Agricultural Economics* also contain illustrations of changes in roles.

Clearly, excellence in disciplinary and subject matter research is necessary, but not sufficient, for useful problem-solving analysis. The ability to perform useful problem-solving research demands advances in analysis. Undue attention to the discipline of economics for its own sake leads to a neglect of useful analysis. Thus, the world agricultural economics research establishment needs to give continued attention to achieving a proper balance of the roles of disciplinary, subject matter, and problem-solving analysis.

Conclusions

Introspection is helpful in charting future directions and needed corrections whether by individuals or organizations, and within limits, its benefits promote resourcefulness. This article has been one professional's view of the status and condition of the agricultural economics research establishment. A brief essay like this one cannot attempt a comprehensive assessment of the details of agricultural economics. Instead, my four assertions represent my personal perspective and observations.

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Economic Theory in Agricultural Economics Research

Emery N. Castle

An inevitable tension prevails between the application of a science and its parent discipline. The worker who wishes to address actual problems must use a general theory that is often ambiguous when applied in certain circumstances. Few contemporary agricultural economists would deny the importance of economic theory, but opinions vary about its use generally and the most effective specific theory for addressing particular problems.

Agricultural economists use as well as contribute to economic theory. A few examples of theory contributions serve to make this point.

T.W. Schultz and J. Kenneth Galbraith are agricultural economists whose work is of such generality that they have been claimed by the parent discipline of economics. Both have held professorships in recognized university departments of economics, and both have served as president of the American Economic Association. Schultz is a Nobel Laureate in economics. Others, Fred Waugh, Mordecai Ezekiel, and Elmer Working, for example, have done research that was promptly recognized in the parent discipline and became a part of mainstream literature. Marion Clawson's theoretical insight on demand for outdoor recreation led to an enormous literature in recreation economics. Original work by other agricultural economists was recognized only after comparable discoveries had been made by economists. The work by Heady and Dillon preceded by a decade comparable developments on duality in economics (Berndt and Field, p. 3).[1] Halter, Carter, and Hocking's note on the transcendental production function anticipated the "translog" production function in current use. Mark Regan's contribution in using welfare economics as a theoretical base for benefit-cost analysis was recognized by only a few contemporaries doing comparable work. In Regan's case, it probably could not have been otherwise because of the nature of his USDA appointment. As a member of the interagency committee that developed the controversial "Greenbook," he was not free to publish his work independently until considerable time had passed. By then others had covered the same ground in more academically oriented literature.

Castle is professor and chair of the University Graduate Faculty of Economics at Oregon State University, Corvallis. I thank the following people who advised me on this essay: Richard Adams, Olvar Bergland, Steven Buccola, Clark Edwards, Richard Johnston, Bruce Rettig, Bruce Weber, and Gene Wunderlich.

[1]See complete citation at end of article.

Intensity of Use of Economic Theory

Agricultural economic literature varies with respect to the intensity of use of economic theory. At one extreme, theory is used only as a general guide to inquiry by assisting in delineating problems, isolating major variables, and suggesting cause and effect relationships. Empirical investigations resulting from such an approach may be, but are not necessarily, highly quantitative. Here, one's primary intent is not to question the applicability of economic theory, but inadequacies or anomalies may be noted and preserved in the hope they will be addressed in subsequent theoretical investigations. This general use of theory does not permit theory to be "tested" except in a most indirect way.

The above approach is not in style today. A closer correspondence between theory and real world conditions is now believed more appropriate. This view, which stems from logical positivism, holds that theoretical concepts are valid only if they can be quantified or lead to theoretical propositions that can be quantified. For applied research purposes, theoretical concepts without major modification may be adequate for real world situations (in effect, theoretical propositions are assumed to have empirical content). In such circumstances, the purpose of the investigation is not to question deductions from theory but rather to apply or illustrate them in a particular setting. Rigorous methods may well be employed. Economic models may be specified mathematically, elaborate surveys may be conducted, and sophisticated quantitative techniques may be used. But the employment of such methods will not ensure that either the empirical content of economic theory will be questioned or economic phenomena will be better understood. Theory may be rescued from reality in several ways: models may be reformulated, variables defined differently, significance levels adjusted, and different data sets tried.

The most intensive use of economic theory in applied research assesses the correspondence of theoretical explanations or predictions with reality. An attempt is made to judge whether the investigation has led to an improvement in real world understanding beyond that implicit in the theory. And here is the rub. How is this to be decided, or how does one know that better understanding has been achieved? Neither the philosophy of science nor the methodology of economics provides a ready answer.

Research Methodology in Applied Economics

Logical positivism and logical empiricism dominated the philosophy of science for a period and have had a major influence on the methodology of many disciplines, including economics. Under logical positivism, theoretical propositions are to be tested or confirmed by experience. Many scientific advances have emerged from the research of those who used, or believed they were using, this approach, but there has been a steady erosion in the adherents of this point of view within the philosophy of science. Karl Popper proposed that positivism be modified by requiring that theoretical propositions were scientific only when capable of being falsified. Thomas Kuhn reacted to Popper by saying falsification is not the way science is practiced and questioned whether science could be practiced that way. Others, for example, Peter Feyerabend, have argued against a general methodology in science altogether. A more moderate position is that of Imre Lakatos, who believes science proceeds in the context of research programs, rather than by crucial tests of particular hypotheses. He sets forth criteria for judging if research programs are progressive or degenerative (see Blaug; Caldwell; and Hausman for summaries).

These developments in the philosophy of science have not gone unnoticed in economics. A journal, *Economics and Philosophy,* established in 1985, examines methodological approaches in economics by the use of philosophy as well as economics. Recent books on methodology in economics include Blaug; Caldwell; and McCloskey. Although the three agree on one point (that economists give far more lip service to falsification than can be justified by the use they make of it), their prescriptions for the improvement of economics are quite diverse. Blaug would have economists give greater attention to falsification—they should practice what they preach. Caldwell recommends pluralism: alternative approaches for the explanation of economic phenomena should be pursued and compared. McCloskey is more critical of the rhetoric of economics than of its content. He argues that economists should be more explicit about their methodology. If they were, he believes they would utter fewer brave words about falsification and rigorous tests of theoretical propositions.

Such matters have received some recent attention by agricultural economists. In 1985, Alan Randall organized a session and presented a major paper at the annual meeting of the American Agricultural Economics Association on alternative theoretical approaches utilized in natural resource economics. Yet, one can hardly say that agricultural economists are preoccupied currently with the philosophical or methodological base from which they work. This contrasts with their considerable familiarity with quantitative methods, including operations research techniques and data processing.

Theory and Reality

A theory may be said to be rich in empirical content if it predicts or explains real world phenomena well. The applied economist may assume a theory and use it for a particular purpose rather than test it. For example, the estimation of welfare gains and losses from a particular market intervention is likely to carry with it many theoretical assumptions, some explicit and some implicit, about the way the world actually is.

Few practicing economists would argue, however, that available theories are adequate for all purposes; the applied economist has a major stake in history improvement. Such improvement can occur in at least two ways. One is by a deductive process: making a theory internally consistent with as few premises as possible, given the objectives of the theory. The other is to enrich its capacity to predict or explain (improve its empirical content). Applied economists may be able to make significant contributions in this respect because they typically work with information based on real world experience. Developments in the philosophy of science and the methodology of economics in the past two decades suggest that it would be fruitful for agricultural economists to establish a dialogue on how the empirical content of the theory they use might be improved. Agricultural economists make numerous conjectures, projections, and predictions about events yet to occur. In even greater abundance are explanations of past events, which range from the qualitative to the highly quantitative. But neither set of activities, standing alone, is likely to have much impact on the empirical content of a theory.

The two should be combined. When projections, predictions, or conjectures are made, the raw material for an additional investigation is at hand. For example, when the supply response of a type of farming area is projected, the projection can become a hypothesis for subsequent research. If the projection missed what actually happened, why did the projection err? Were input prices incorrectly specified? Were the coefficients close to what prevailed? Were the behavioral assumptions highly suspect? Conversely, historical explanation of past events can be used to make predictions for comparable future conditions. The announcement of new policy initiatives often creates a laboratory for judging the reliability of explanations of past comparable events.

If this kind of rigorous exposure of theory to reality is to occur, agricultural economists will need to establish an attitude with respect to anomalies or failures of their theories to explain or predict. On Bayesian grounds, it might be argued that existing theory incorporates past discoveries, and that failure of a theory in a particular case pits this past accumulation of knowledge against a single discrepancy. Under such circumstances, the discrepancy may be treated as an anomaly and dismissed. But, if all anomalies are dismissed, the so-called accumulation of knowledge clearly is biased because only confirmations are taken into account. The attitude of the profession regarding the accumulation of discrepancies or anomalies becomes fundamental. Will they be accumulated systematically and conscientiously, or will they be dismissed or ignored? My colleague, Steven T. Buccola, has likened a body of theory to an open access resource. If the incentive system facing applied workers or theory users is inappropriate, the body of theory may be depleted rather than enhanced. If incentives are biased to favor the publication or preservation of theory successes only, a biased view of a theory will develop over time. If failures as well as successes are noted, there is hope a theory will become more robust or that alternative theoretical explanations will be advanced.

On occasion the awards program of the American Agricultural Economics Association has seen fit to recognize research that reported anomalous results. For one example see the award winning publication by Edwards and others. Journals serving agricultural economics should establish explicit policies with respect to the publication of anomalies or theory failures.

Alternative Theories

The prevailing theoretical orthodoxy in agricultural economics is that of neoclassical equilibrium economics. The recent publication of *The New Palgrave: A Dictionary of Economics,* however, calls our attention to the enormous range and diversity in theoretical formulations upon which the agricultural economist may draw. A contemporary classification system would include neoclassical economics, institutional economics, radical or Marxian economics, and Austrian economics (Caldwell and Randall). Surely the adoption of such a classification system is to paint with a broad brush. Not only are there theory subsets within these broad classifications, but concepts from one approach may be utilized within an alternative framework. While overlap among alternative theories may exist, direct comparison of theories is exceedingly difficult. The philosophy of inquiry underlying different theories may not be the same, and it may be inappropriate to apply the same standards to all (Caldwell, chap. 13).

Except for the advantages of specialization, there is no fundamental reason agricultural economists should confine themselves to one theoretical formulation or approach. The benefits of individual specialization are considerable, however, and the costs of pluralism may be high. To learn rigorously the prevailing orthodoxy requires demanding intellectual effort. When a graduate student has done so, there may be insufficient time in a graduate program to acquire an understanding of alternative theories. The result may be research that repeatedly uses a particular theoretical formulation. The mechanical use of the same theoretical formulation may be combined with sophisticated empirical procedures with no provision for the identification, preservation, and subsequent investigation of anomalies or discrepancies. This may create the impression of an advanced scientific enterprise even when there is no accommodation for knowledge growth.

Greater eclecticism in the use of economic theory would improve agricultural economic research (Hausman). This does not necessarily imply the rejection of neoclassical economics. This body of thought has been able to survive because it is capable of alternative interpretations. When a particular formulation of the neoclassical model fails to explain, predict, or give a desired result, it can often be rescued by another plausible neoclassical formulation. For an example, note the great variation in recommendations for a rate of discount on public works projects, many of which have been derived from alternative neoclassical models (Lind). One may note the different policy implications of alternative neoclassical models applied to regional development (Weber and Deaton).

No tract on the use of economic theory for applied research would be complete without mention of the normative base of economics. Economists need not be wedded to a particular normative position such as utilitarianism. Greater flexibility in this respect may make the results of economic analysis more acceptable to noneconomists, a matter of some importance to those numerous agricultural economists who do research designed to evaluate public policies.

Much public policy research in agricultural economics is based on Pareto optimality models or Kaldor-Hicks compensation tests. The appeal of these approaches is that they appear to avoid the necessity of making interpersonal utility comparisons, but Pareto superiority tests provide no guide as to which parties will reap the surplus that results from moving from an "inefficient" to an "efficient" arrangement. Furthermore, the distribution of income and wealth is not usually a variable when such analyses are made. Respectable alternative approaches include the use of social welfare functions as well as "rights"-based

frameworks. The works of the philosophers Rawls and Nozick provide examples of alternative approaches.[2]

According to Rawls, the fundamental rules in society should be derived from behind a veil of ignorance. While Rawls was not the first to use the veil of ignorance technique, it is his formulation that is receiving the greatest attention. Rules may be said to be "fair" if they are formulated by people who have no knowledge of their particular circumstances with respect to such matters as conditions of birth, nationality, race, or generation. By use of this technique, Rawls comes to the conclusions that reorganizations are fair only if those in the least advantageous position benefit from the reorganization. This is not the place to argue the merits of the Rawls position but rather to establish it as an alternative to Pareto-based rules (see Baumol; Varian; and Wunderlich for examples of economic research on fairness).

Nozick's work is at the opposite philosophical pole. He believes individuals have rights and any infringement of those rights is morally unacceptable. According to Nozick, the minimal and maximal state that can be justified is that state which protects people's property rights. As a consequence, people may not be indifferent as to whether goods are produced by the public or private sector, as would be the case for the utilitarian. In the jargon of the welfare economist, rights are in the nature of nonwelfare information. Such information may be incorporated in social welfare functional forms, in function parameters, or as side constraints. Clearly, the imposition on an economic system of this kind of nonwelfare information will have a major impact on outcomes, as would the requirement that "fairness" conditions be met.[3]

Summary and Conclusions

I make a plea for the systematic treatment of anomalies in research and for pluralism in the use of economic theory. Every agricultural economist, of course, is unlikely to become knowledgeable in all facets of economic theory. Nevertheless, a case can be made for greater diversity of theoretical approaches. This applies to alternative uses of neoclassical economics as well as to the employment of concepts from Marxian, Austrian, and institutional economics.

Much of the current policy research done by agricultural economists is greatly influenced by Pareto-based criteria for comparing alternative policy outcomes. Research results stemming from this normative base may not always be persuasive. Users of such research may be unwilling to accept the implicit Pareto version of fairness, or they may not be indifferent with respect to the role of the state. The issue here is not that of selecting the "correct" normative stance but rather of recognizing that alternative normative positions exist and are capable of being incorporated into economic research.

[2]The final part of this essay has benefited greatly from conversations with Steven Buccola.

[3]Nozick's position does not exhaust the possibilities of the rights-based approach. Welfare state entitlement to certain necessities might also be viewed as a right imposed on an economic system.

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On the Role of Data and Measurement in Agricultural Economics Research

James T. Bonnen

Agricultural economics was established as an empirical science. Its capacity, credibility, and resources are attributable to its capability as an empirical science and to its relevance to society's needs. This tradition and our reputation as agricultural economists are based on a balanced emphasis of (1) theory (including disciplines other than economics), (2) statistical and other quantitative measurement techniques, and (3) data. This is the three-legged stool that supports empirical tradition.

I believe that our profession has increasingly celebrated and rewarded theory and statistical methods while ignoring data (Bonnen, 1988).[1] Consequently, we are undermining our capacity as an empirical science and as a profession.

Claiming a body of inquiry to be a science depends on the grounds on which its knowledge is asserted. Empirical science depends on a theoretical statement of causation supported (or, more properly, not disproved) by empirical evidence. That evidence is formed around the same concepts as the theoretical explanation of causation. Data must be defined in the same terms as the theory being supported or negated. For the empirical test to be valid, the act of measurement must be logical, consistent, and appropriate to the measurement problem faced.

Agricultural economics appears to have devoted far too little attention to its data in relation to its theory and formal measurement processes. The ultimate basis of acceptance of a scientific theory is consensus, which occurs when theory is consistent, tests are valid, and empirical results are supportive. Consensus depends on the way a theory squares with a real world described by its data. The specification of that data requires the same underlying causal logic and rigor demanded of economic theory and statistical methods (Churchman).

Types of Research

Research serves multiple purposes and its products take many forms. For such purposes as research design, funding strategies, and data collection and management, it is convenient to distinguish three broad classes of research—disciplinary, subject matter, and problem solving.

Disciplinary research is the theory, empirical measurement, and the measurements techniques and methods used to explain a fundamental class of phenomena of concern in such disciplines as chemistry, microbiology, economics, or philosophy. Expanding such knowledge increases the capacity of a discipline to explain nature and human behavior. Data are necessary to any consensus about theories that make up a discipline.

Two other types of research are of an applied, multidisciplinary nature. Subject matter research combines different disciplinary products into knowledge useful to a set of decisionmakers who face a common set of problems. Agriculture is not a discipline but a subject matter, as are its subsets, such as animal science, agronomy, agricultural economics, and farm management. Practical decisions are often difficult to make based on general subject matter knowledge. A better informed decision depends on problem-solving research that further processes disciplinary and subject matter information into information more directly relevant to the specific problem on which a decision must be made. Problem-solving knowledge takes a form that is relevant to a single decisionmaker (or set of decisionmakers) who has a specific practical problem on which action is necessary. The processing of data and information produces a continuum in which distinctions differ in degree and purpose.

Problem-solving research differs from disciplinary and subject matter research in that it always seeks prescriptions, that is, "should" or "ought" statements that depend on values as well as on relatively value-free knowledge. Value data are frequently missing from agricultural economics research, and additional attention both to the content and form of value data would do much to enhance the quality of research and usefulness of the prescriptions. The acceptance of a production innovation, for example, may depend more on the values of producers or consumers, or on the rules that structure and govern the decision process, than on the technical qualities of the innovation.

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[1]See complete citation at end of article.

The Nature of Data, Information, and Knowledge

Data are symbolic representations of concepts, quantities, and actions and are the direct product of measurement or counting. Information is more. It usually combines data from different collection processes and subject matters always within some analytic interpretation. Interpretation may range from little more than formatting of data for presentation, to encoding in an index or scale, to modeling complex economic, engineering, and biological phenomena. Information is data that are processed, organized, interpreted, and communicated. The information's usefulness aids decisionmaking or subject matter evaluation, whether in science or in the practical world (Bonnen, 1977).

Finally, when one speaks of a body of (disciplinary or subject matter) knowledge, one is referring not just to tested (validated) information but tested information around which a scientific/professional community consensus has formed. Until a broad consensus of appropriate scientists accepts the validity of an information set, it is not generally treated as part of the corpus of knowledge in that discipline or subject matter.

Philosophies of Knowledge

Data, information, and knowledge, in a sense, form a continuum ranging from raw sense experiences to carefully catalogued wisdom.[2] A researcher's view of the role of data arises from that researcher's philosophy of knowledge. The positivists' concept argues that the only descriptive knowledge that can be objective and therefore scientific is value-free knowledge. Philosophers discredited extreme forms of positivism decades ago, rejecting the possibility of totally value-free knowledge. Physical scientists and most biological scientists have since cast off the limitations of logical positivism, but, paradoxically, it persists in the social sciences and still tends to dominate agricultural economics (Castle).

The premises of a positivistic philosophy of knowledge influence the data collected and its interpretation. Excessive positivism has not only resulted in a deficiency of value-oriented data, but it has narrowed the interpretation of available data. Observing the level of investment in soil conservation measures, for example, reveals only evidence of purchase and installation as relevant and objective measurements, not the farmer's attitudes, understanding, peer pressures, and goals.

Descriptive, positivistic knowledge is partially acquired through the five senses and is analytic (logical and conceptual) as well as synthetic (descriptive). It combines theory (statements of causation) with undefined primitive terms known through experience and interaction to produce contingent descriptive (empirical) statements about the perceived reality of nature and human and other behaviors (Johnson, pp. 41-53).

There are several other philosophic positions of consequence in social science research, including normativism and pragmatism. Clearly, one's philosophy of knowledge will dictate what is considered admissible as scientific evidence. A strictly positivistic approach will tend to exclude normative statements about what is good or bad, even if such statements are descriptive and, thus, factual.

Normative statements (about goodness and badness) can be regarded as empirical or descriptive statements. Thus, both positive and normative philosophic positions require data appropriate to their causal theories. Like value-free knowledge, value knowledge can be viewed as experiential, acquired through the five senses. Consequently, the same tests of truth used in positivism can be applied to knowledge of goodness and badness to support the claim of an objective, descriptive knowledge of values. These tests assess correspondence (experience), logical coherence (internal consistency), and clarity (the proposition to be tested is not ambiguous or vague and thus can be tested) (Johnson, pp. 41-64).

Both positive (relatively value-free) and normative (value) knowledge ultimately depend on a leap of faith that the five senses reflect something real in nature and are not perceptual illusions. Thus, both knowledges depend on philosophic "primitives," undefined terms known from experience (for example, good/bad, hot/cold). Neither value nor value-free knowledge may be regarded as knowable with certainty. All knowledge is contingent in science and thus subject to revision. A modern or balanced view would allow both positive and normative statements to be tested empirically for relations to theory.

Another view, that of the pragmatist, argues that value-free and value knowledge are interdependent in their consequences and that attempts to establish a clear distinction between them are arbitrary and mistaken. The truth of knowledge is viewed as dependent on its practical consequences. Thus, truth is instrumental and dependent on the use of knowledge. The ultimate test of truth is workability, although

[2]This section is an edited version of a similar discussion in (Bonnen, 1989). In both versions, a major intellectual debt is owed to Glenn L. Johnson, who has contributed greatly to my education and others' in the philosophy of science and philosophic value theory.

coherence and clarity are relevant *ex ante* tests of pragmatic prescriptions, the form in which most pragmatists frame any inquiry. This philosophic value position tends to be held by most experienced policymakers and, within the colleges of agriculture, by most extension staff and some problem-oriented teachers and researchers. Indeed, it is the typical philosophic value position of problem-solving (Johnson, pp. 65-75).

In my view, the philosophic ground on which one chooses to stand to address a particular inquiry should depend not only on the specific purpose of the inquiry, akin to pragmatism, but also on different philosophic positions which should be combined as appropriate to address various parts of complex inquiries (Johnson, pp. 22-7, 221-35). Many of the arguments among agricultural economists, even alleged disputes over empirical evidence, arise out of differences (often unconscious) in their philosophic views. We need to be more conscious of these differences and their advantages and disadvantages in different kinds of inquiries.

The Value of Research

The value of information from research is derived from its value in decisionmaking, whether in science or in practical problems. The value of information in a decision depends on the extent to which it is news to the decisionmaker. The value of new information is the value of the decision made with that information minus the value of the decision without it and minus the cost of the new information (Bonnen, 1977). Data collected as news has utility in decisions, with most of the utility going to the market participant who acts first.

Some Bad Habits

Research from an information system point of view acknowledges complexity and seeks balanced attention to all elements of inquiry. Failure to acknowledge the interdependence of all the elements of inquiry can result in some bad habits, which can be destructive of effective research and professional performance. These bad habits include:

An exclusive or excessive emphasis on one of the three legs of the empirical tradition in science to the exclusion or detriment of the others. The most common is an excessive focus on theory development without an adequate empirical test of the theory. Some use no data at all. They just publish mathematical proofs of the logical consistency of the theory. Others use inappropriate secondary data formed for a different purpose around concepts that differ significantly from those to be tested. Sophisticated econometric technique is then used in an attempt to compensate for the weaknesses (Leontief).

The growing lack of experience with primary data collection in agricultural economics. The rising economic value of time and the labor-intensive nature of data collection are part of the reason for the lack of experience. But often the only accurate way to test a concept is with data collected specifically for the purpose.[3] The profession is slowly losing touch with the problems involved in the designing and processing of data, which in turn are critical elements needed in selecting and modifying appropriate data for modeling.

Lack of extensive hands-on experience with agricultural subject matters. This was not a problem in earlier times. Most agricultural economists then grew up in rural communities or on farms and brought some substantial command of agronomy, animal science, and other relevant fields and disciplines to graduate training in agricultural economics. This is no longer the case. Few modern agricultural economists see any need to learn much about complex multidisciplinary subjects before modeling or doing other types of research on them. One cannot use the research of other disciplines or collaborate effectively with scientists in other fields without adequate understanding of the relevant fields and disciplines. The capacity to judge accurately the correspondence between concepts and reality is not what it used to be in agricultural economics. This, however, is offset somewhat by greater knowledge in other areas, but there is a limit to the substitution possibilities.

Failure to do sufficient preliminary data analysis in preparing for modeling, forecasting, and other analytic processing of data. That is, economists do not work directly with the microdata sets sufficiently to learn the strengths and weaknesses of the data they are preparing to use. Rather, they plunge ahead, working with various (usually secondary data) aggregates that often obscure many of the weaknesses and characteristics of the data.

Excessively narrow and inflexible philosophic commitment (to logical positivism, for example, or a narrow normativism). This limits one's view of the world and choice of data and methods in research. Failure to let research purpose guide epistemological choices can constrain and distort the quality of one's research as well as one's judgment of other research.

[3]See (Bonnen, 1988) for a discussion of possible reasons for this decline in primary data collection.

The common but arrogant belief that the only legitimate, respectable, or useful research role for agricultural economics is disciplinary research. All else (subject matter and problem-solving research) is second-rate science—a view common to the elitism of some basic scientists. This attitude has produced an equally erroneous reaction in some agricultural economists who will tell you that the only legitimate role is limited to subject matter and problem-solving research. Both views destroy the balance of commitment needed for effective performance in agricultural economics research. This lack of balance is not limited to agricultural economics and is undermining the capacity of and commitment to the land grant idea in many colleges of agriculture. The large number of institutions in trouble with their legislature and clientele attest to this and other difficulties.

The output of agricultural economics that generated our resources and societal support included disciplinary, subject matter, and problem-solving information. We now tend to focus so much on the discipline of economics that the profession's incentives and capacity for producing subject matter and problem-solving information is slowly eroding. With this comes an erosion of relevance to society's needs and eventually the support of research. The profession needs the multidisciplinary capability to produce subject matter and problem-solving knowledge.

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Agricultural Ethics and Economics

Paul B. Thompson

After decades of mutual disinterest, philosophers and economists have suddenly discovered extensive areas of overlap in their disciplines. An upsurge of interest in the ethics of agriculture has also taken place in the past decade. These two events are largely unrelated, however. Agricultural economics research needs to bring these two strands of literature together in the coming decade. Agricultural policy analysis is a promising area for this sort of work.

At least three different developments in philosophy and economics have brought the disciplines together. First, philosophers have become increasingly interested in rational choice theory, having come to see it as central to the analysis of many problems in ethics, political theory, and the philosophy of mind. Second, economists have rediscovered the relevance of culture and norms in forming the institutions requisite for economic exchange. Third, philosophers and economists have fought a series of battles over the analysis of public policy. For some time, it appeared that these battles would produce only acrimony, but as policy professionals have become more sensitive to the strengths and limitations of both economic and ethical theory, bringing both to bear upon the analysis of policy has become possible. These three areas, in turn, have precipitated an upsurge of interest in the traditional philosophy of economics, the result being many fine books, articles, and at least one journal, *Economics and Philosophy*.

Research in agricultural ethics has different origins. The publication of a series of popular books and essays criticizing agriculture first stimulated research on conceptual and ethical issues that seemed to be at the root of the criticisms. A group of rural sociologists then began to address ethical issues directly as part of an attempt to create a new sociology of agriculture. Philosophers with an interest in applied ethics and public policy began to identify world hunger and animal welfare issues as themes for a series of books and articles beginning in the early 1970's. The relevance of these themes to agriculture was, from the philosophers' perspective, accidental, but they provided a foundation for more systematic research and teaching on agricultural ethics.

Although these two broad developments, the disciplinary bridging of philosophy and economics, on the one hand, and the rise of agricultural ethics on the other, have different origins, their convergence clearly creates both an opportunity and a responsibility for agricultural economics research in the coming decade. Agricultural economists should build upon the work from the past two decades to revitalize some existing research within the discipline and to initiate important new areas of research. I shall not say much about the rising disciplinary overlap between philosophy and economics here. Hausman and McPherson (1990) have addressed some of those points, and my readers may consult some of the sources I have noted if they wish to learn more.[1]

Agricultural Ethics in the 1980's

Although recent professional work in agricultural ethics has been conducted by practitioners of many disciplines, rural sociologists and philosophers have a plurality of the entries in the emerging literature. The primary outlet for this work is the professional journal, *Agriculture and Human Values*, and more recently, *The Journal of Agricultural Ethics*. The topics covered have been quite varied, including issues of risk and consent in food safety, questions of fairness regarding U.S. agricultural labor, and attempts to understand "sustainability" as a norm. The general areas that have received the greatest coverage are the farm crisis of the 1980's, the emergence of biotechnology in agriculture, and the internationalization of agriculture. The farm crisis issues are predictable: the moral status of "family farms," compensation for failed farms, and analysis of responsibility for structural change. The issues of biotechnology span a wider area: environmental and esthetic influences, farm structure effects, and impacts upon the organization and management of agricultural research. Topics in internationalization of agriculture are more diverse still, ranging from critiques of "green revolution" approaches to agricultural development to debates on the goals for agricultural policy in the European Community.

There is no doubt, however, that Rachel Carson, Jim Hightower, Frances Moore Lappe, and Wendell Berry deserve most of the credit (or blame) for stimulating philosophical research on issues in agriculture. Prior to the publication of books such as *Silent Spring* or *The Unsettling of America*, work on agricultural ethics was pursued by a coterie of rural social scientists whose work, though sophisticated, was largely ignored by professional philosophers. The popular critics were more difficult to overlook. They blasted an agricultural

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[1]Sources are cited in the References section at the end of this essay.

establishment that had wallowed in self-praise since World War II and which had come to regard moral purity as a birthright. The establishment's reply to critics often began and ended with the claim that they were not part of (and hence could not know anything about) agriculture.

The existence of social conflict was itself interesting to sociologists, but the establishment's failure to meet the terms of the critics' arguments really precipitated most philosophers' interest in the debates. Rarely does a defender of agricultural practice challenge the factual claims of a critic. Far more typically, critics and defenders talk past one another, applying different concepts and values to their different understandings of the situation. These kinds of conceptual confusion are keenly relevant to ethical and political theory, and conceptual controversies perpetuate conflicts that philosophers are trained to analyze and, perhaps, resolve. The result: research on agricultural ethics has tended to follow controversial topics. This research has not been driven by any comprehensive method or theory but by subject matter. As a result, many of the generalized attempts to state "what ethics can offer agricultural economists" (including more than one that I have written myself) are not very good. The ethics literature is far better at talking about the problems than at talking about itself. The most productive approach, therefore, may be to examine the 1983 farm crisis as a case study in agricultural ethics.

The Farm Crisis and the Economic Analysis of Agricultural Policy

The farm crisis of the 1980's has substantially different coverage in the writings of critics than in the agricultural economics literature. Noting these differences will illustrate the kinds of philosophical presumptions that ethicists want to understand. Agricultural economists have labored to document farm structural change, hoping to find variables that might explain this change. The task involves the use of production and sales data to classify U.S. farms. The bimodal analysis of U.S. farm structure reveals that low-volume, part-time operations are relatively healthy in financial terms, and the number of high-volume, capital-intensive operations are growing. Other farms (the middle group) that fall in between are declining in numbers, in their share of total farm production, and in profitability.

The bimodal analysis reveals certain ethical implications. The decline in the number of farms in the middle group can readily be interpreted as a "cost" of farm policies, usually understood in terms of the aggregate financial losses and emotional stress suffered by individuals who are forced to make adjustments involuntarily. This interpretation is well suited to a framework in which alternative policies are evaluated in a general comparison of outcomes, the relative costs and benefits of each policy proposal. Such comparisons reveal tradeoffs among the policy choices, for example, how policies that mitigate costs in terms of farm stress and financial loss can be expected to impose higher costs in other areas, to taxpayers, perhaps, or to consumers. This approach has misleadingly been called utilitarian. A true utilitarian, however, would insist upon policies that optimize the ratio of benefit to cost.

Many agricultural economists recognize that the political acceptability of policy costs will be influenced by many factors that, on the face of it, at least, appear to contradict the utilitarian insistence upon optimization. Luther Tweeten (1983, 1987), for example, has acknowledged that the family farm's historical role in the U.S. national heritage provides a valid reason for accepting less than the optimal ratio of conventional costs and benefits.

The philosophy that I attribute to economists is consequentialist, however, in the sense that it is the expected value of policy outcomes (the costs and benefits) that are compared in making an evaluation of policies. If farm structural change is seen as a crisis, we must make the ethical judgment that the total number of individual farm failures in the middle group represents unacceptable costs for public policy. This judgment may be unacceptable because the policy does not produce compensating benefits for farms in the middle group, or it may be unacceptable because the costs are too great in absolute terms without regard to compensating benefits. Decisions that produce monetarily suboptimal outcomes may be justified for a variety of reasons, but policy criteria that can be expressed in terms of expected value are consequentialist criteria.

Alternative Views of the Farm Crisis

Critics who have expressed concern about the plight of family farms during the past two decades have tended to bring very different philosophical frameworks to their understanding of the farm crisis (Thompson, 1988a). Burkhardt has published a perceptive analysis of the debate. He shows that one group of critics, including Jim Hightower and Marty Strange, have argued that recent events in agriculture constitute a crisis because they represent a foreclosure of ethically important opportunities in American society. Their view is associated with a longstanding defense of capitalism against Marxist criticisms. The standard philosophical justification of *laissez-faire* capitalism has always been that it best achieves the ideal of Government by consent of the governed. In contrast, Marxists have long held that wage workers in industrialized capitalist economies are denied autonomy because they sell their labor (themselves) to survive. Populist anticommunists have argued that American society escapes this charge because of its agrarian base. Farming was an open opportunity to all Americans, one in which they might live a poor, hardscrabble

existence, to be sure, but one in which they would be autonomous. They would "be their own boss."

Hightower and Strange see the transitions taking place in the U.S. farm sector as a crisis, not because lots of individuals are adversely affected but because the political legitimacy of *laissez-faire* capitalism is being eroded. Ironically, many economists who have attempted to respond to this criticism have mistakenly presumed that the populists are enemies of capitalism. True, the populists sanction intervention in markets for land and for agricultural commodities, and this intervention may sacrifice allocative efficiencies. The populists do not value capitalism or markets for their efficiency, however, but for their uncompromising protection of private property and individual autonomy.

From a philosopher's perspective, the populists seem to be more strongly committed to capitalism than their detractors. Populists attribute intrinsic value to capitalist institutions of property and free exchange, while many economists see them as merely instruments for assuring efficiency and growth. The populists would rather be free, but poor, than be forced to gain wealth working for bosses (not to say that they object to wages—only that the individual must have a meaningful alternative to them). By a happy coincidence, to a populist, unregulated markets also promise economic growth. Efficiency arguments, on the other hand, seem to place no intrinsic value on private property, sanctioning rather severe interference in personal autonomy when market failures can be demonstrated. Consumer sovereignty becomes a means for efficiently allocating society's resources. It is the greater satisfaction resulting from free transactions that justifies markets for the consequentialist. To the populist, consequentialism places too little emphasis upon personal autonomy.

Burkhardt finds a second kind of argument in the writings of Wendell Berry (1977), and in many of the statements made by religious leaders (Comstock). The argument is difficult to summarize, and I think that many of my economist colleagues have underestimated its force because they have limited themselves to summarized versions like Berry's (1987) or the one that follows here. The idea is that one's life activity is as potent as one's will in forming moral character, that human beings have a moral and religious duty to cultivate virtues and to shun vices, but some life roles are more conducive to this than others. Aristotle thought that only aristocrats would have the wealth and leisure time needed to cultivate the virtues, but American philosophers, like Ralph Waldo Emerson, argued that virtue is best learned by living a life in which an individual's roles are well integrated with one another and with the natural environment. Traditional farming was thought to be virtuous because family roles were integral to the economic life of the farm, and the farm itself was integrated into the cycles and requirements of nature.

Given this background, the farm crisis has nothing to do with declining numbers of farmers, with financial or emotional stress, or with the costs and benefits of farm policy. The passing of the well-integrated, virtuous life is a crisis, not only in that few can live this life of virtue, but also in that virtuous rural life disappears as a role model for those in the city to look to for inspiration. Traditional farms are not valued as instruments for producing virtue. They are valued because they are experiential prerequisites for even conceptualizing duty and virtue.

Therefore, we have a duty to preserve traditional farms (of which even Berry admits there are now few). The duty is not conditioned upon calculating the costs and benefits of doing so. This simple duty is, in this respect, like a traditional religious duty. The Christian's duty to follow God's commandments is not generally thought to be derived from the fact that following commandments produces more benefits than harms. Duty to God is a simple, direct duty, not a duty done for the sake of the consequences produced. The natural law tradition of ethical theory holds that public policy must facilitate the performance of natural duties and must never controvert natural duties.

While many people would reject the philosophical framework in which Berry develops his views on the traditional family farm, most people understand what he is talking about, which, oddly, does not hold for agricultural economists. In at least two published debates, Wendell Berry has advocated his perspective on farming against consequentially oriented agricultural economists. The issue of who "won" the debates, I think, depends upon the values one brings to reading them. What is relevant here is that Berry's interlocutors seem conceptually incapable of dealing with the family farm issue in anything but consequentialist terms. They accuse Berry of emotionalism and irrationality. It is one thing to be convinced that one's own philosophical perspective is right; it is quite another to be so closely wedded to it that one excludes the possibility for rational disagreement on philosophical frameworks.

My point is to show that the bimodal analysis of farm structure change analysis is more attuned to one philosophical approach to ethics than it is to others. Economists are not biased in the sense that they favor specific policies (though some do). Neither is the bimodal analysis biased in the sense that it favors specific interest groups. Yet, the view that the farm crisis should be understood in terms of how it affects producers exiting the middle group of farms has resulted from agricultural economics research which is not philosophically neutral. Other ways of approaching public policy place comparatively little emphasis upon how a policy influences conventional economic variables, without evaluating policy in terms of measurable costs and benefits.

Agricultural Ethics, Agricultural Economics, and Agricultural Policy

I would not endorse either the populist view or Berry's view against the conclusions of the analysis that has been favored by agricultural economists. My point in discussing the views has been to present alternatives to the consequentialist framework favored by agricultural economists, and to demonstrate the philosophical assumptions of the standard approach in agricultural economics. When the possibility of alternative policy criteria is understood, a host of important problems can be more effectively grasped. Berry and the populists, for example, prefer certain kinds of institutional arrangements regardless of the monetary consequences of adopting them.

Agricultural ethics is relevant to the institutional component of policy analysis in at least three distinct ways. First, ethics are, in one sense, institutions that have a profound effect on the performance of markets. Moral norms establish property constraints and entitlements that an economist can ill afford to ignore (Thompson, 1987). Second, as implied in my discussion of farm crisis literature, alternative philosophical frameworks can evaluate, justify, or legitimize a given policy (Thompson, 1988b). Even if one is philosophically committed to choosing policies because of the consequences they produce, one would hope that a competent, professional policy analyst would have an intellectual grasp of the rights-based, communitarian, and procedural alternatives.

The third area of relevance is more deeply philosophical. Philosophers like Kant and Rawls have tried to develop a way of asking a question that probably never occurs to most of us but whose answer is vitally important to the shaping of our public laws and policies. The point of departure is Knight's observation (Buchanan) that the kind of people we are—what we believe and desire—is strongly determined by the moral norms, the opportunities, the legal structure, and the daily practices of the society in which we live. Knowing this, how can we shape our society so that it allows us to become the sort of people that we ought to be? The question requires us to strive for a kind of objectivity that may seem paradoxical. Rawls' famous thought experiment, "the original position," is intended to present a method for approaching the question by shedding all the information that individuals have about their particular wants, desires, and life goals, but by retaining all that we know about human nature and society (including economics!), which is needed to fashion an answer. By addressing our philosophical question we can arrive at a deeper grounding for consequentialist, rights-based, communitarian, or procedural theories for evaluating public policy.

Conclusions

Wendell Berry's work on the family farm is, in my view, pointed toward this third area of relevance, one that might be called "constitutional choice," in the sense implied by Anthony Giddens. Berry wants communities that produce certain kinds of people. He thinks that rural communities of America's past did so. He is, on my reading, less interested in preserving farms than he is in preserving the philosophical values of a farming people. This preservation requires a defense of those values and of the institutions that produced them. That defense, in turn, requires an attack upon the new techniques, technologies, and management strategies that inform a farm producer's choices and form the next generation's values. The new agriculture is, on my reading of Berry's work, undermining the constitution—understood as the work habits, loyalties, space-and-time awareness, and community coherence—of American society.

Again, I will shy away from endorsing Berry's view, for I am far from sanguine about the constitution of traditional rural America. Although lack of space prevents a defense of my views, the reader should know that I think there are good historical and normative reasons why any of the authors writing on constitutional choice (Castle) provide more promising strategies than does Berry. Berry's work is important because it demonstrates the necessity of raising deep philosophical questions about agriculture. Rural social scientists neglect it at the peril of confining themselves to shallowness.

The potential for new knowledge in the three areas I have noted is great and can be enhanced by interdisciplinary research in agricultural economics and agricultural ethics. Cross-disciplinary work requires more cross-referencing of the literature, and cross-referencing means that agricultural economists will have to start reading agricultural ethics. The full potential for interdisciplinary work will not be realized until there is more collaboration among economists, philosophers, and interpretively oriented sociologists. This will require some institution building of our own, and we have a long way to go.

The American Agricultural Economics Association announced a section for contributed papers on agricultural ethics for this 1990 meeting, but none were submitted. More than 100 scholars, but only a handful of economists, attended the 1989 meeting of the Society for Food, Agriculture, and Human Values. These two incidents indicate that agricultural ethics and agricultural economics may be ships passing in the night. Neither subdiscipline can afford to continue in that vein.

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Agricultural Economics and the Chaos of Economic Methodology

Daniel M. Hausman and Michael S. McPherson

How should agricultural economists go about their business? If they are unfortunate enough to read some economic methodology (and it is hard to avoid doing so, since without warning, it creeps into introductory paragraphs and reflective asides), they are bound to feel confused. There is something like "received wisdom" which says that positive and normative economics must be radically divorced and that economists have really nothing to add on normative questions except better facts and better positive theory. The standard view of positive economics is much like Milton Friedman's—one wants correct and useful predictions, and one is uninterested in anything else. Philosophically inclined methodologists such as Mark Blaug are more circumspect, but the received wisdom is nevertheless that economists should stick to positive economics and should look for theories that work predictively.

On top of this vague orthodoxy, and only partly conforming to it, is the insistence of high theorists (particularly in the rational expectations school) that economists must seek structural models in which the behavioral postulates are all derivable from fundamental microeconomic theory. Despite the apparent, methodologically *laissez-faire* implications of the view that only predictive success matters, Friedman and the methodologically orthodox are virtually as insistent as the high theorists that one avoid any behavioral assumptions that cannot be shown to result from individual maximization.

Donald McCloskey more radically repudiates methodology altogether. Like Blaug and others, McCloskey points out that economists do not practice what the received methodological wisdom preaches. But, he then argues for the striking thesis that there is no reason for economists to follow any methodological preaching at all. According to McCloskey, the standards of good argument in economics are whatever standards of good argument economists accept.

All this must be baffling to agricultural economists, who cannot afford to indulge in pure abstract theory or to plunge deeply into issues in the philosophy of science. Economists have little choice except to imitate the methodological practice of their teachers and colleagues. But reflective individuals, who are concerned about their goals and want their practice to contribute to those goals, cannot avoid wondering whether there is something worrisome or potentially useful in this methodological ruckus.

So what ought one to think about economic methodology? Here are a few simple remarks along with brief hints at supporting arguments.

1. Received wisdom proclaims that not only are positive and normative questions distinct, but on matters of values only grunts and clubs are called for, not civilized and rational argument. This view is absurd and destructive, because people argue about even the most emotionally charged ethical issues, and their arguments are not completely without effect. There are intellectually respectable ethical problems, and an education in economics should not disqualify one from contributing to their solution. Sound assessment of economic policy alternatives requires defensible moral foundations as well as correct positive analysis.

Some economic theorists have made important contributions to foundational questions in moral theory in recent years, but applied economists do not need to become rare philosophers to cope constructively with the ethical dimensions of their work. In fact, the tools economists use in their daily work, such as cost-benefit analysis, discounting of future returns, and assessment of externalities, are morally defensible as part of the normative apparatus of a pluralistic liberal society. Forthrightness in acknowledging the normative commitments behind policy analysis leads not to nihilism but to a more critical awareness of both the strengths and the limitations of these tools.

2. Friedman's view is generally taken to be that one should regard economic theory as a set of tools that does some jobs well. If a hypothesis works, no questions need be asked about whether its assumptions (basic propositions) are realistic (true). If a hypothesis doesn't work, scrap it. If, analogously, a computer program solves some problems correctly, one might say, "Why worry about the correctness of the algorithms upon which the program is based?" But, the analogy shows what is wrong with Friedman's view. To know that the program solves all the significant problems correctly, without assessing the algorithm and code, one would need to know the answers already, and one would not need the program. To say that a hypothesis

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"works" means only that it has worked in some sample. This success may give good reason to believe that the hypothesis will work in other cases, but one may also want to examine the assumptions (the algorithm of the program). In that way, one may anticipate successes or failures in new applications. When hypotheses or programs fail, the most efficient way of modifying them involves examining the realism of their assumptions or the peculiarities of their algorithms or codes. So, agreement with Friedman's practical, predictive vision of economics does not imply that one should treat theories as black boxes. One should take seriously questions about the realism of assumptions and the truth of irrelevant or uninteresting predictions.

3. With their insistence on structural models, the rational expectations theorists go part of the way toward the view defended in the paragraph above, for they agree that mere predictive "success" is not necessarily a good basis for policy. They do not agree however, that one should take the assumptions of models literally and seriously and attend to their empirical weaknesses. Instead, the rational expectations theorists exalt the basic model of individual rationality and self-interest to a status little short of *a priori* necessary truth. Their work results in theories with striking elegance, system, and unity, and their efforts are justified, for there is a good deal of truth to the image of agents with complete and transitive preferences motivated by nothing except the size of their own bundle of commodities and services. But such a picture is obviously not the whole truth. Other behavioral generalizations, no matter how disconnected from or even inconsistent with the basic model, may sometimes be a better basis for prediction and explanation.

4. Economists do not behave the way that Blaug or Friedman say they ought to. But Blaug and Friedman are mistaken about how economists ought to behave, and McCloskey is wrong to argue that (apart from the norms of honest and polite conversation) there are no rules—that anything goes that in fact persuades. We cannot sort through the relevant epistemological issues in a paragraph or two, nor do agricultural economists need to master this complex area of philosophy in order to do their work. What we would instead suggest is this: (a) that agricultural economists reflect on the kinds of arguments they make and heed (as McCloskey rightly urges); (b) that agricultural economists reflect about what they aim to achieve in particular inquiries and what constraints they face; and then (c) that agricultural economists consider what sort of arguments could rationally help achieve their goals, given their constraints. If all is as it should be, the arguments that, according to step a, in fact persuade are the ones that, according to step c, ought to persuade. But all is not, of course, necessarily as it ought to be in this very much less than best of all possible worlds.

5. Economists do many different things and, except perhaps at the highest level of abstraction, the same methods of theory construction and appraisal do not apply to all activities. In thinking about what theories to consider, how to appraise and modify them, and how much credence to give them, agricultural economists need to take into account the goals, constraints, and data peculiarities that apply to their activity. (Since we are not agricultural economists and do not know the discipline in detail, we cannot formulate or address the most pressing, practical, and important methodological questions that agricultural economists face.)

Although methodological reflection could not radically and rapidly change agricultural economics, even if it needed changing, such reflection is unavoidable, potentially confusing, but conceivably valuable. Methodological reflection is unavoidable, because agricultural economists who are serious about their work must consider what they want to accomplish and what is the best way to accomplish it. Methodological reflection is most likely to become confusing when it is abstract and purely philosophical, because economists for the most part are not competent epistemologists. Such reflection is most likely to be valuable when it is undertaken in relationship to the practical problems and objectives of inquiry.

General Equilibrium Analysis of U.S. Agriculture: What Does It Contribute?

Thomas W. Hertel

Applied general equilibrium analysis as we know it today has intellectual origins in the debate over the feasibility of the centralized computation of a Pareto optimal allocation of resources within an economy (Whalley, pp. 30-34).[1] During the first half of the century, economists were preoccupied with the question of whether or not it was computationally feasible to solve the associated system of behavioral equations. Recent developments in operations research have proven optimists correct. It is indeed possible to solve very large models representing national economies, either in the form of centralized planning problems or, more commonly, as decentralized equilibrium problems. While this has not brought an end to the debate over the operational relevance of general equilibrium theory, the increasing use of computable general equilibrium (CGE) models in policy analysis has served to sharpen the debate. It now focuses heavily on questions such as model specification, parameter choice, disaggregation, and the appropriate representation of policies (Whalley). CGE analysis now has a great deal in common with partial equilibrium modeling, a theme I will develop in this essay.

Leif Johansen implemented the first operational CGE model in the late 1950's. Variants of this model are still used for planning purposes in Norway (Shreiner and Larsen). These models have also been popular in the development economics literature (Robinson, 1988, 1989). CGE analysis in North America has tended to emphasize tax and trade issues (Shoven and Whalley). One consequence is that the U.S. Department of Treasury and the Canadian Department of Finance both have employed variants of these models for a number of years. However, nowhere has CGE analysis been as successfully institutionalized as in the Australian Industries Assistance Commission (Powell and Lawson; Vincent), where it has been used for 15 years to analyze the economywide effects of changing relative rates of protection.

In light of this history, the relatively recent appearance of CGE analysis in U.S. agricultural economics may be viewed as a belated arrival. But, perhaps, there is good reason for this. A general equilibrium model is only as strong as its partial equilibrium components, and, until recently, most CGE models had fairly simple representations of producer and consumer behavior. Thus, further acceptance of CGE models applied to U.S. agriculture hinges on better communication between the modelers and other agricultural economists aimed at strengthening critical components of the models. Unfortunately, partial equilibrium analysts who do not understand CGE models cannot improve them.

This essay attempts to bridge the gap between partial and general equilibrium analysts, thus promoting greater dialogue between practitioners of CGE analysis and others in the profession. Along the way, I hope to dispel several misperceptions of CGE analysis. For example, CGE analyses are often viewed as abstract, theoretical exercises, with little resemblance to partial equilibrium modeling, much less to the real world. While some CGE applications may fall prey to this criticism, there is no inherent reason why this must be the case. To show the similarities between the partial and general equilibrium approaches, I will begin by comparing partial and general equilibrium analyses of a subsidy on farm output. Similarities and differences as well as strengths and weaknesses of the two methodologies will be identified.

Partial Equilibrium Analysis of a Farm Subsidy

Partial equilibrium models may be thought of as collections of supply and demand equations, representing a summary of economic behavior in various markets of interest. Since agricultural economists are often concerned with farm commodity markets, the supply equations in many models describe the supply of products from the farm sector, while the demand equations describe the market conditions facing producers beyond the farmgate. Depending on the particular biases of the researchers, these supply and demand equations may or may not be derived from explicit assumptions about producer and consumer behavior. Let's leave that issue aside for the moment and turn to the implications of this framework for analyzing a farm subsidy.

I will simplify by assuming that the intervention in question may be expressed as an *ad valorem* output subsidy. (More complex policies do not shed any light on the distinction between partial and general equilibrium analyses.) In this case, the relationship between the farm price of output (p_F) and the market price (p_H)

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[1]Sources cited in the text are listed in the References section at the end of this essay.

is simply $p_F = s * p_H$, where $s > 1$ denotes the presence of a subsidy. The partial equilibrium solution to this problem, written in terms of the supply and demand elasticities, becomes:

$$\hat{p}_H = (\eta_F - \eta_H)^{-1} \eta_F(-\hat{s}), \quad (1)$$

where "^" denotes proportional change. In the case of a single commodity, η_F and η_H are the relevant supply and demand elasticities, so that we may verify the familiar special cases whereby: (a) either perfectly elastic demand ($\eta_H = -\infty$) or inelastic supply ($\eta_F = 0$) results in the subsidy benefits going entirely to farmers, while (b) either perfectly elastic supply ($\eta_F = \infty$) or inelastic demand ($\eta_H = 0$) results in all of the subsidy benefits being passed on to consumers. In the multiple-commodity case, s is a vector of interventions, and η_F and η_H are elasticity matrices, with as many dimensions as there are markets in the model.

This multicommodity, partial equilibrium framework is adequate for many, if not most, applications. However, there are some important gaps to keep in mind. A first limitation of expression 1 is its failure to acknowledge the finite resource base in the economy. To the extent that the farm subsidy encourages resources to move into agriculture (or perhaps it discourages them from moving out), the rest of the economy has less land, labor, and capital to work with. There is an opportunity cost associated with this factor movement that is only superficially acknowledged by the introduction of upward sloping factor supply schedules into a partial equilibrium model of the farm sector.[2]

A second limitation of the partial equilibrium model is its failure to address the question: Who foots the bill for the added subsidies? The opportunity cost of raising 1 dollar of additional revenue via the current system of Federal income taxes has been shown to be considerably more than a dollar, due to the marginal excess burden associated with further distortions in consumer and producer choices (Ballard; Shoven and Whalley). By contrast, this phenomenon may be explicitly dealt with in a CGE model. Even if an economywide model does not flesh out the tax system, it is possible at least to keep track of the transfers implicit in a given subsidy.

The third weakness of the partial equilibrium model is the absence of an explicit budget constraint for the household(s) in question. There is no link between the sources and the uses of income. Changes in factor returns are not reflected in altered consumer expenditures. This limitation is most severe when the policy shocks considered are very large, and when they result in income transfers between households with very different consumption patterns.

A final limitation of the partial equilibrium approach is the absence of any definitive check on the conceptual and computational consistency of this model. The larger and more complex the model, the greater the probability of inconsistencies. This leads to skepticism on the part of model users and potential "consumers" of model results. By contrast, as will be pointed out below, Walras' Law offers a powerful check on the consistency of a well-defined general equilibrium model. In fact, this alone may be reason enough to justify the CGE approach in some cases.

General Equilibrium Analyses of a Subsidy

To illustrate how the general equilibrium approach addresses these limitations, it is necessary to lay out the structure of a very simple CGE model. Because the model is only complex enough to illustrate my basic point, it can be completely described by one picture (fig. 1).

A Simple Model

I have assumed a closed economy, with one aggregate household which consumes two goods: food (good #1) and nonfood (good #2). This household also owns all primary factors of production in the economy, which are held in fixed supply ($\bar{q}_{H0}$ and $\bar{q}_{H3}$). Again, for the sake of simplicity, I distinguish between only two primary factors: farmland (good #0), which is specific to the farm and food sector, and a capital/labor aggregate (good #3), which is mobile between the two sectors. Household income ($y = p_{H0}\bar{q}_{H0} + p_{H3}\bar{q}_{H3}$) is derived from these primary factors. Given the initial household budget shares, $c_{H1}y$ is spent on good #1 and $c_{H2}y$ is spent on good #2.

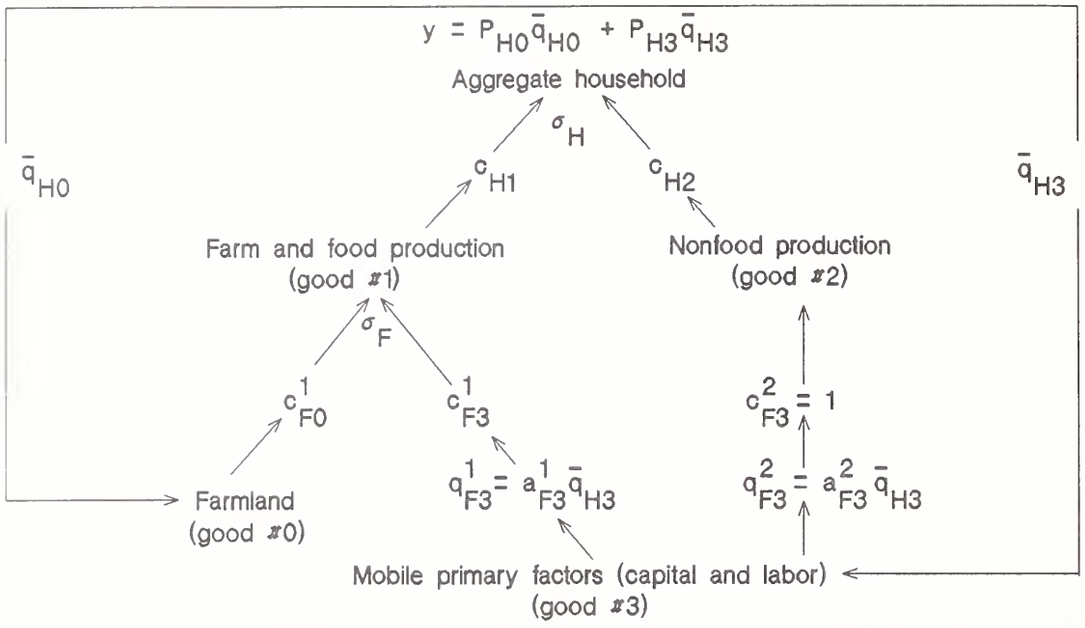
There are no intermediate inputs in this economy, and constant returns to scale in production are assumed. The nonfood commodity is produced using only the capital/labor input, while the farm and food commodity is produced by combining the capital/labor input with farmland. (In a sense, this is what distinguishes the farm economy from other sectors.) In initial equilibrium, the cost shares of each of the two inputs are given by c^1_{F0} and c^1_{F3}. The land and nonland inputs are substitutable in food production, with the ease of substitution determined by the non-negative elasticity σ_F. This substitutability, in fact, underpins the food sector's partial equilibrium supply response in this model, which may be expressed as:

$$\eta_F = \sigma_F(c^1_{F3}/c^1_{F0}). \quad (2)$$

Note that supply response increases with increased substitutability for the fixed land input, and with an increased share of variable inputs in total costs (c^1_{F3}).

[2]See Floyd for an early illustration of this type of partial equilibrium model.

Figure 1
An illustrative general equilibrium model with four goods, one household, and two productive sectors[1,2]



1/ Subscripts H and F refer to the household and firms, respectively. Each of these entities may face different prices for the four goods, which are indexed by using subscripts 0 to 4. Superscripts 1 and 2 refer to (1) the farm and food sector and (2) the nonfood sector. $\bar{q}_{H0}$ and $\bar{q}_{H3}$ represent the fixed supplies of land and nonland inputs which earn returns of P_{H0} and P_{H3}. c^1_{F0} denotes the cost share of good #0 (land) in sector 1, with similar definitions for c^1_{F3}, c^2_{F3} c_{H1}, and c_{H2}. $a^j_{F3} = q^j_{H3}/q_{H3}$, which is the quantity share of this input in sector j.

2/ Each arrow corresponds to an equation in the model. A complete set of equations, along with their manipulation and solution, is available from the author.

The importance of comparing supply elasticities across models is rarely lost on partial equilibrium analysts; however, general equilibrium modelers are less diligent in this matter. This is probably because η_F does not enter directly as a parameter in the model, but must instead be calculated as in equation 2.[3] Based on my own experience, differing assumptions about partial equilibrium behavior go a long way toward explaining empirical discrepancies between partial and general equilibrium models. Many differences between such analyses are falsely attributed to "general equilibrium" effects. Consequently, I encourage partial equilibrium modelers and other consumers of CGE results to demand this type of summary information from CGE modelers.

A final point regarding figure 1 stems from the fact that we need a convenient method of summarizing the two sectors' relative claims on the mobile factor of production. This may be done by defining $a^1_{F3} = q^1_{F3}/q_{H3}$, that is, the quantity share of the capital/labor aggregate which is used in agricultural production. Similarly, $a^2_{F3} = q^2_{F3}/q_{H3}$.

[3]See Hertel (1989) for a general discussion of how to compute partial equilibrium supply elasticities under alternative assumptions about technology and factor mobility.

Introducing a Farm Subsidy

Having laid out the basic notation for this model, we are in a position to solve it. This involves collapsing the equations implicit in figure 1 into as many dimensions as there are commodities. In this sense, finding a solution is no different than in the partial equilibrium case, with one important distinction. Since the general equilibrium model is exhaustive in its treatment of economic activity, one of the market-clearing conditions is redundant. This is Walras' Law, which has two important practical implications for CGE analysis. First, since we are left with only three equations, we can solve only for the changes in relative prices among the four commodities. Here, I will choose land as the numeraire good and will omit the supply = demand equation for land, which will produce the following outcome after introduction of a farm subsidy:

$$\hat{p}_H^* = \begin{bmatrix} p_{H1}/p_{H0} \\ p_{H2}/p_{H0} \\ p_{H3}/p_{H0} \end{bmatrix} = \begin{bmatrix} 1 + (c^1_{F3}x) \\ c^2_{F3}x \\ x \end{bmatrix} (-\hat{s}), \qquad (3)$$

$$\text{where } x = \frac{(a^1_{F3}c_{H2} - a^2_{F3}c_{H1})\,\sigma_H}{-a^1_{F3}c^1_{F0}\sigma_F - (a^1_{F3}c_{H2} - a^2_{F3}c_{H1})\,(c^1_{F3} - c^2_{F3})\sigma_H}.$$

The second practical implication of Walras' Law is that if we evaluate changes in supply and demand in the omitted land market, using the equilibrium relative price changes, they must be equal, that is, $\hat{q}_{H0}(\hat{p}_H^*) = \hat{q}_{F0}(\hat{p}_H^*)$. Since this property is an implication of the entire model's structure, its verification offers a global check on the conceptual and computational consistency of this model. This can be extremely valuable. By way of example, consider what might happen in a large nonlinear model with hundreds of markets when a new wrinkle such as imperfect competition is introduced. What if the modeler forgets to distribute to households the excess profits generated in the new equilibrium? How could we possibly know that this has been omitted? There is no general method of checking for such overlooked inconsistencies in a partial equilibrium model. However, in a general equilibrium model, such a leakage would result in insufficient commodity demand, and hence, insufficient derived demand for land. Consequently, Walras' Law would be violated.

Now return to expression 3. Note that each of the relative price changes depends on the common parameter "x." This parameter is actually quite similar in structure to the ratio of elasticities premultiplying the farm subsidy in expression 1. To see this, note that (given c_{Fj} and c_{Hj}) σ_F determines η_F and σ_H determines η_H. Thus, the value of x, and the subsequent degree to which this subsidy is shifted among markets, depends fundamentally on the supply and demand elasticities embedded in this model. When $\sigma_H = 0$, such that demands are not responsive to price, or $\sigma_F = \infty$, such that the partial equilibrium farm supply curve is perfectly elastic, $x = 0$ and all benefits from the subsidy are passed forward to consumers. This is the same result we encountered in the partial equilibrium case. By contrast, if nonland inputs cannot be substituted for the sector-specific land input ($\sigma_F = 0$), then farm supply will be completely unresponsive to price, and x may be shown to collapse to $x = (c_{F0}^1)^{-1}$, which means that all of the subsidy benefits are passed back to landowners.[4] This confirms our partial equilibrium intuition.

We can also see how, in the more general case, all the parameters in the model have a bearing on the general equilibrium outcome.[5] Note, however, what happens when the farm sector becomes "small" relative to the rest of the economy. The value of a_{F3}^1 approaches zero, and we can obtain approximate relative price changes for the farm sector without reference to the nonfarm economy:

$$(p_{H1}/p_{H0}) \cong [1 + (c_{F3}^1/c_{F0}^1)](-\hat{s}), \text{ and}$$

$$(p_{H3}/p_{H0}) \cong [1/c_{F0}^1](-\hat{s}). \quad (4)$$

This is a formal demonstration of why we do not need a general equilibrium model to assess accurately the farm sector impact of most farm policy changes in the case of a highly industrialized economy such as the United States. This point likely comes as little surprise to many economists. Since just 2-3 percent of the U.S. labor force is involved in farming, why model the entire economy to say something about the agricultural impact of a grain subsidy?

Three Common Myths Dispelled

At this point, I hope that I have clarified several popular misconceptions about CGE modeling. The first myth is that general equilibrium analysis is an abstract, theoretical exercise. However, applied work in this area boils down to a problem of constructing a sound data set and choosing (or estimating) appropriate elasticities. This is hardly a theoretical exercise, and it is really no different from partial equilibrium modeling.

A second myth is that CGE models are complex and difficult to solve. In fact, the solution of CGE models is not necessarily any more complex than the solution of large, nonlinear partial equilibrium modes. Matrix inversion is sufficient to solve a locally linearized CGE model. And the solution of well-specified nonlinear CGE models can generally be accomplished by choosing from a variety of software alternatives. For large models, the CGE approach has the great advantage of having a global consistency check, namely Walras' Law.

The third myth relates to the value of indiscriminate applications of CGE analysis to issues facing U.S. agriculture. For example, if we can conduct CGE analyses of mandatory supply control, then why not look at more specific issues like the tobacco program? Again, if one is interested only in how a farm policy intervention influences the farm sector itself, then partial equilibrium analysis is probably good enough. Rather than investing scarce time and money in developing (or modifying) a CGE model, it may be much wiser to devote research resources to the improvement of partial equilibrium analyses.

I imagine the reader is now wondering: What's the big deal? Why all of the fuss about CGE analysis of the farm and food economy? Keep reading—the next section discusses why such efforts are warranted.

[4]The magnification factor, $(c_{F0}^1)^{-1} > 1$, serves to load a given amount of subsidies onto the smaller land expenditure base.

[5]The reader might wonder why income elasticities of demand do not appear in expression 3. Since the economy in figure 1 is closed (that is, there is no international trade), and with fixed factor supplies, the only way disposable income can change is through a change in the efficiency with which the fixed resource base is employed. Such changes are commonly referred to as changes in "excess burden." In this context, I have assumed the economy in figure 1 to be initially in an undistorted state, where firms and households face the same prices ($p_{Hn} = p_{Fn}^1 = p_{Fn}^2$ for all n) prior to introduction of the subsidy. This means that, to a first order approximation, marginal changes in the farm subsidy will not have excess burden effects. Since there is no change in excess burden, then there will be no change in household income. This is why income elasticities of demand do not appear in the solution.

Benefits From Using CGE Analysis

Five good reasons exist for applying the CGE framework in selected analyses of U.S. agriculture:[6]

Accounting consistency
The treatment of interindustry effects
Theoretical consistency
CGE analysis as a vehicle for "putting things in perspective"
Welfare analysis

Those who have worked with such models quickly recognize that the accounting identities in a CGE model are as important as the behavioral equations. The fact that: (a) households cannot spend more than they earn, (b) the same unit of labor cannot be simultaneously employed in two different places, and (c) the economy as a whole must balance its payments with the rest of the world, serves to circumscribe the range of possible general equilibrium outcomes. CGE models are built upon a social accounting matrix (SAM) which details all the basic identities for a given economy (Pyatt and Round, Hanson and Robinson). Thus, the first advantage of CGE analysis revolves around the explicit incorporation of these accounting identities into the behavior model.

A second advantage of CGE analysis arises from tracing and measuring interindustry linkages between the farm and nonfarm sectors. When conducting a partial equilibrium analysis, it is often very difficult to know where to "draw the line," because the farm sector purchases inputs from the manufacturing, mining, and service sectors, and farm output is sold to both food and nonfood sectors. Having an exhaustive model for analyzing such issues is valuable.[7]

Theoretical consistency is particularly important in large economic models, which too often become black boxes, unintelligible, even to other modelers. Unless individual components are built upon received economic theory, other researchers have great difficulty interpreting model results. If the model structure adheres strictly to standard neoclassical theory, as do well-specified CGE models, the model user can draw upon experience to understand and explain what is going on when a given policy is changed. A final advantage of theoretical consistency, which has already been mentioned, is that Walras' Law may be used as a global consistency check.

Another advantage of CGE analysis is that it helps to put things in perspective. Microeconomic theory emphasizes the importance of relative, as opposed to absolute, levels of economic variables. How taxation or technological change affects the farm sector is largely irrelevant unless we know how the levels of these variables compare with those in the nonfarm sector. For example, taxation of the farm and food sector reduces farm output when viewed in isolation. However, a general equilibrium analysis of U.S. tax policy reveals that low relative rates of taxation have conferred an implicit subsidy on agriculture which at times has rivaled direct farm payments in overall importance (Hertel and Tsigas).[8]

Finally, one of the things I like best about CGE models is the fact that they force us to focus more clearly on households, and, ultimately, on people. Changes in welfare are measured by examining the change in household utility, or the implied changes in real income (adjusted for all price changes). The latter is much more concrete, in my opinion, than the concepts of producer and consumer surplus, and emphasizes, for example, that farmers are consumers and taxpayers as well as owners of agricultural assets.

Conclusions

In an attempt to catch up with three decades of applied general equilibrium analysis, U.S. agricultural economists have recently risked overdoing CGE modeling. We may have oversold the benefits of CGE modeling, while failing to acknowledge many of limitations.

Yet, there remain areas of use for CGE models in agricultural economics which have not been pursued with sufficient vigor. One of these is integrating diverse research results and bringing them to bear on specific policy problems. Where else can the work of a production economist be integrated with that of someone estimating complete consumer demand systems?[9] Many research results in international trade, industrial organization, and marketing may also be integrated into a CGE framework. This makes it possible to distinguish between (a) parameters that we either already know a lot about or which are unimportant for current policy issues, and (b) parameters about which we know relatively little but which are key to the accurate assessment of important policies. The implications of such model-sensitivity analyses for directing future policy-oriented research should not be overlooked.

CGE analysis has also received far too little attention in the training of graduate students. Too many Ph.D. agricultural economists graduate without acquiring an economywide perspective on agricultural problems.

[6]See Hertel (1990) for a survey of CGE applications relating to agriculture.

[7]Input-output analysis has been the preferred vehicle for assessing interindustry linkages. CGE analysis simply permits us to relax the customary assumptions of fixed I-O coefficients, perfectly elastic factor supplies, and exogenous final demand. In this sense, I-O analysis is a special case of CGE analysis.

[8]See also Boyd, and Boyd and Newman for further analysis of tax policy and U.S. agriculture within an economywide setting.

[9]See Hertel, Ball, Huang, and Tsigas for an illustration of this point.

They often lack the ability to see how their specialized knowledge of production, consumption, marketing, and trade fits together and how these diverse pieces interact in determining the incidence of shocks to the farm and food system.[10]

In summary, CGE analysis is a valuable addition to the agricultural economist's analytical tool kit. However, a CGE model is only as good as its individual components. Robert Solow made this same point in his critique of Jay Forrester's global modeling efforts in the early 1970's. Forrester asserted that rather than "go to the bottom of a particular problem ... what we want to look at are the problems caused by interactions." To this, Solow (p. 157) responds:

> I don't know what you call people who believe they can be wrong about everything in particular, but expect to be lucky enough to get it right on the interactions. They may be descendants of the famous merchant Lapidus who said he lost money on every item he sold, but made it up on the volume.

Future improvements in the CGE analysis of U.S. agriculture depend on developments in the partial equilibrium specification of farm sector models. Thus, a great need for dialogue exists between those who are primarily involved in CGE modeling and those who conduct other sorts of farm sector analyses. However, such a dialogue can be fruitful only if CGE models are "demystified." It is my hope that this essay will help to further such debate within the agricultural economics profession.

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[10]A copy of a syllabus for such a course at Purdue is available from the author. Write to: Thomas W. Hertel, Department of Agricultural Economics, Purdue University, West Lafayette, IN 47907.

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Doing Agricultural Economics

Clark Edwards

Researchers in agricultural economics seek to describe and explain certain social problems and to prescribe remedies based on available data and theory. I suggest that if you were to watch agricultural economists at work at randomly chosen times you would find them doing one of three things: describing, explaining, or prescribing. And they would be using at least one of three elements or tools: problems, data, or theory. I want to talk about the way agricultural economists describe, explain, and prescribe. In doing so, I must mention research methods, but this is not another treatise on methods.

Each of the three processes can become a specialty. Some researchers are better at one process than at others. While a complete research project involves all three, the tasks can be split and coordinated among specialists, some of whom describe or explain while others prescribe. An individual in control of the entire research process does not have much of a coordination problem beyond that of self-discipline. However, research specialization in large institutions, such as government agencies and the land grant college system, requires coordination as a key to success.

Description Relates Data to Problems

In the descriptive process, researchers arrange selected data to tell a story and illuminate a social problem. I distinguish in this article between social problems for the sake of which research is undertaken and disciplinary problems encountered by researchers along the way. A researcher may ask how much farmers earn compared with nonfarmers or what the distribution of income among farmers is. When dealing with such problems, the researchers may encounter intermediate problems to solve. These are disciplinary problems, such as what theory to apply and what statistical methods to use.

Description is done with the problem in mind more than theory. Successful description, including prediction, can result even when the describer's theories are wrong. Theory, though not necessary for description, can help in pointing the way to useful results, and can sometimes suggest changes in the description. Bonnen (*3*) illustrates what goes wrong with the research process when the descriptive phase is shortened.[1] He says agricultural economists have increasingly celebrated and rewarded theory and statistical methods while ignoring data, thus undermining their capacity to do research.

The social importance of a problem addressed by research determines the relevance of the project. Choosing a problem is a subjective process, and the selection can be made in many ways. The researcher may choose a problem, the general public may express concern, or professional politicians and administrators may select and rank problems for the agricultural economics research agenda. The researcher may select data in many ways as well, through primary or secondary sources. A research project built on faulty data or focused on an irrelevant issue will be valueless, regardless of the descriptive ability of the researcher.

Explanation Relates Theories to Data

Explanation of a social problem comes out of the interaction of data with theory. Theory relates facts and concepts. For example, the data may show a correlation of Government price supports with land values. The theory helps determine which is cause and which is effect.

We use theory to take apart the descriptive material and recombine it to see what might have been. Adam Smith illuminates this point when, after noting in his first few pages that the wealth of nations depends on the quantity of labor and the output per worker, he asks what happens when the number of workers changes (through population growth) or when the productivity of workers changes (as in the pin factory) (*13*).

Arguments over the best balance of theory and data have a long history. A resolution was sought in econometrics during the first half of this century. Some econometricians leaned toward pure theory and deduction from accepted premises; others leaned toward brute force empiricism while minimizing the role of theory. When theory failed to correspond to data, one school said the theory must be wrong, the other said the data must be wrong. Castle shows how agricultural economics literature varies in the intensity of use of theory—from highly empirical to highly theoretical (*4*).

McCloskey avoids this conflict by turning against method (*9*). Instead, he treats research as a conversation: the rhetoric of economics can be examined in the same way as the rhetoric of literature. McCloskey helps to humanize what others treat as an objective process, but he diverts us from examination of research methods.

Edwards, an economist, served on the editorial board of this Journal from 1966 to 1989 and was editor during 1976-83.

[1]Italicized numbers in parentheses cite sources listed in the References section at the end of this article.

Hausman and McPherson, on the other hand, suggest that reflection on method helps (*5*). We do not need to be diverted from a study of method, they say, because too few economists concern themselves with method already. Perhaps no one method is right, but reflection on choices among alternative methods can help us avoid the many wrong ones.

Different Explanations Use Theory Differently

Much of the discussion of method comes down to where one stands on the centuries-long argument between brute force empiricism and pure theory. No wonder we can't solve it with one more journal article, or even one more book.

Farm management researchers emphasized the empirical approach in agricultural economics just after the turn of the century. Warren (*15*) and others drew on the teachings of Pearson (*10*). Production economists, who studied the same problems that farm managers studied, but who called farms firms, emphasized theory. Heady (*6*) and others drew on Black (*2*).

One group said, "Look at the data before conjecturing about hypotheses." For example, Sherlock Holmes studied all the facts first, then formulated the only hypothesis consistent with those facts. The other group said, "Formulate your hypothesis from theory before collecting data with which to test it." It took a while to become comfortable with researchers who worked both sides, who studied data for clues and theories for hypotheses, and who let progress on either front influence actions taken on the other. Not until probability theory became the basis for econometric procedures after World War II did researchers use systematic procedures to test various plausible theories with imperfect data by using probability inference.

The argument over data and theory goes on, but in different forms now. Researchers debate on model size, the degrees of abstraction, whether to use general or partial formulations, and the proper theoretical underpinnings.

Hertel points out that it is now possible to solve large models (*7*). Now that it is possible, the question arises: is it better? Can we build better models using 25-50 years of full-time equivalent work over a 5-10 year period than one person can build in 6 months?

The answer, of course, depends on what you are trying to do. Sometimes smaller models are appropriate, sometimes larger ones. It is only since larger models became feasible that the subtleties of the argument took on more meaning. Before, the failure of large models could be attributed to lack of funds, poor administrative organization, or lack of technology. Only after several projects had surmounted these obstacles could the efficacy of the larger models be judged.

Strands of the argument between the brute force empiricists and the armchair theorists live on in the debate over specific models versus general ones. Those who favor general models say the balance leans toward abstract models that are easy to understand and to generalize into a broad range of applicability. In focusing on relationships among facts and concepts, such models develop general knowledge about how the agricultural economy works.

Some researchers lean toward models that are concrete and specific. Through a high degree of effort in capturing accuracy of details, they describe and explain the real world. Such models develop knowledge about dealing with particular problems in the agricultural economy.

When smaller models were technical necessities, researchers often preferred partial equilibrium to general equilibrium models because they could not maintain the detail needed to understand certain aspects of the agricultural economy through the whole model. There were models for pork and others for dairy, but no models for the livestock sector. Models for feed grain were isolated from models for food grain. Trying to include a livestock sector in a model that already handled the crop sector could end a research project. Some models focused on commodity markets, some on factor markets, and others on retail sales. Researchers building models that embraced the whole agricultural sector, and perhaps integrated agriculture into the whole economy, had two choices. If much empirical detail was included, the logic of the model had to be simple (as input-output models are). If the theory was complex, size could be held down by making the model abstract.

Hertel shows that computer general equilibrium models are sometimes more appropriate than the more popular partial equilibrium models because they can examine aspects assumed constant in partial models (*7*). He says computer general equilibrium models can be applied; they are not confined, as some of the literature seems to imply, to the abstract and theoretical. With recent improvements in computer hardware and software, large models using general equilibrium concepts are now easy to do. Hertel writes that computer general equilibrium is not always better than partial equilibrium, any more than theoretical models are always better than applied ones. He sets out to bridge the gap between partial and general equilibrium models.

Most modeling done by agricultural economists draws on microeconomic theory. This is true of both large and small, theoretical and applied, general equilibrium

and partial. This choice suits many problems in agricultural economics well, but it means that certain important problems cannot be addressed.

For example, aggregative microeconomic general equilibrium assumes full employment of resources. Macroeconomic theory, on the other hand, recognizes idle resources in stable equilibrium. This is why macroeconomists recognize unemployment in the labor force and idle capacity of plant and equipment, while agricultural economists (using microeconomics) consider farm labor and capital fully employed.

Researchers Judge Among Alternative Theories

The choice of theory used to examine the data and analyze the social problem affects the prescription of how to deal with the problem. There are several objective approaches to evaluation of theory, but in the end, it comes down to a subjective judgment.

Some agricultural economists want theory to represent or imitate reality. Hausman and McPherson express concern for the realism of assumptions (*5*). They warn against raising assumptions to the status of *a priori* truth. They point out that Milton Friedman touted a different and more popular view: if the theory works, use it. Getting the right answer for the wrong reasons does not satisfy Hausman and McPherson.

Other agricultural economists want theory to express a point of view. Hausman and McPherson show how the choice of microeconomic theory reflects the researcher's reliance on rationality and self-interest by firms and households. Researchers who choose macroeconomics may do so to highlight idle resources or to suggest that government intervention rather than free markets can best solve the problem. Regional economists and operation researchers are more eclectic in their choices among theories. They choose a combination from the array of possible theories that will highlight what the researcher judges to be the important elements of the problem.

Others judge theory according to how well it captures formal features of the economy. For example, Hertel shows how to specify conventional microeconomic theory to bring out the important features of taxation (*7*). Others emphasize how government price supports work, or how uncertainty about weather or exports affects domestic farm output decisions.

The usefulness of the theory used to confront the data during the explanatory phase of research can be assessed in four different ways:

- Is the model logically consistent? If the conclusions don't follow from the premises, fix the logical errors.
- Does the model conform to the facts? How high are the correlation coefficients and the t-ratios? If low, you may have to improve your data or modify your theory.
- Are the prescriptions in conformance with social values? For example, if the society uses central planning and the model assumes decentralized decisions, or if the government intervenes in some markets (as with price supports) and the model assumes perfect competition, then change the model.
- Does the model feel good to you, the researcher? Is it emotionally satisfying? If you think this last sounds funny, watch a couple of excited researchers as they struggle with two pieces of chalk and one blackboard searching for a conceptualization of a problem that feels right.

Prescription Relates Explanations to Problems

The use of analysis is an important measure of a profession's effectiveness, according to Hildreth (*8*). Prescription is the end for the sake of which research is done. Research-based prescriptions flow from the descriptive and explanatory processes, depending on the definition of the problem, on the availability of data, and on the choice of theory. A prescription recommends an alternative for action. Budget-conscious national policymakers may be advised, for example, to lower the loan rates on food and feed grains. Or, profit-maximizing dairy farmers may be advised to plant small-grain fields with pasture grasses, expand the dairy herd, and purchase feed grain rather than grow it.

Making a prescription requires that the researcher take a stand on what to do. Many research economists shy away from the precarious role of policy adviser. Even though the prescription may be based on extensive and careful description and explanation, the researcher may become identified with a particular point of view. Followers may be attracted for ideological reasons rather than for the compelling results of the research process. Opposition also may develop for ideological or political reasons.

Economics is not value free. The role of value judgments is most evident in the prescriptive process. Researchers select problems, data, and theories. Judgments suggest how best to describe, explain, and prescribe.

Thompson discusses some new mergers of economics with philosophy, and of agriculture with ethics (*14*). He shows that they come together in policy analysis, that is, in the prescriptive process. Hausman and McPherson also emphasize the normative commitments inherent in positive policy analysis (*5*).

Economists often have trouble dealing with ethical conflicts because their models say the marketplace resolves all conflicts. When environmentalists disagree with commercial agriculturists they may seek legal action. Instead, the theory used by economists says: let the markets determine who is right. When values conflict concerning alternative actions, Robbins once advocated that the task of the economist is to show the implications of each alternative without expressing a personal opinion about what is best (*11*). Even though he later reversed his opinion, his original teaching lives on (*12*).

Prescription requires communication. (So do explanation and description, and researchers prescribe methods for others to use in solving disciplinary problems.) Researchers communicate with decisionmakers and other end-users of the research product by writing about descriptions and prescriptions.

Researchers' writings on disciplinary problems are often (properly) sprinkled with jargon which may appear obtuse to the general reader. But when they write to end-users about descriptions and prescriptions, the communications should be accessible and clear to all. Agricultural economists are notoriously poor communicators on both social and disciplinary problems. The peer review process is able to turn tangled first drafts into communicative third or fourth drafts when the review process is taken seriously by the reviewers and by the reviewee. Barry finds that the publication process works well (*1*).

If an economist talked only to himself or herself while doing economics, the result would eventually become gibberish, as Keynes once noted. He said that bouncing ideas off one another helps achieve relevance and intelligibility.

Research Specialization Requires Organization and Coordination

This article partitions research into three processes: description, explanation, and prescription. The processes deal with three elements: problems, data, and theory. A complete research project uses all three processes (description, explanation, prescription) and all three elements (problems, data, theory). However, agricultural economists can specialize in performing one or another of the processes or in developing one or another of the elements or tools. Research specialization is more likely to appear in larger institutional organizations.

The difficulty of coordination compounds the problems of proficiency in the processes and training in the tools. The mission of the agency may be relevant and the data reliable so that useful descriptions can be accurate and efficient. Researchers may be well trained in economic and statistical theory and practice so that significant explanations can be found. And information specialists may be able to assure a steady flow of description and prescription to end-users. But, if the research institution is to reach its potential, problems with weak leadership, absorption of budget cuts, reorganization, and other administrative obstacles must be handled so that individual researcher initiative is not discouraged.

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Looking Back While Going Forward: An Essential for Policy Economists

Otto Doering

Institutions, individuals, and historical precedents have always been important in economic policy decisions, but economists seldom adequately reflect these influences in their conventional economic analysis of policy. It seems that economists' deepening lack of cognizance of society's broader objectives means they seldom try policy instruments that are really new.

Other Paradigms

A number of social scientists have developed different approaches which analyze public policy. Institutional economists, historians, and political scientists began with descriptive analyses, developing their own premises, analytical systems, and predictive assumptions. Economists have allowed their own concern for increasing quantification and precision to narrow their focus, taking economics away from its roots in the broad social science view of political economy. Public choice analysis has recently provided a halfway house for economists because of its similar approach and many familiar assumptions.

Economists have compensated in different ways for the divergence from the other social sciences. One methodological alternative open to economists is to modify another social science and attempt to incorporate it within the economics paradigm. Recent efforts analyzing welfare-transferring policies and social welfare-increasing policies are good examples.

Incorporating modifications from other social sciences is attractive because problems are sliced in a way that economists can understand and trust, making such an approach a familiar and more user friendly one. Economists can apply a degree of mathematical rigor to this approach, folding something like public choice analysis into the analytics of neoclassical economics. Causality becomes clearer, capable of being demonstrated in the familiar terms of the economics paradigm. Such adaptations, however, keep the analytical scope narrow.

Other alternatives are public choice, political science, or historical analysis without adaptation. This route gains breadth but poses special difficulties for economists. It forces them into incompatible situations. Economists must integrate information on variables that differ from those in conventional economic models, and the two types of information do not always merge easily or conveniently.

In a sense, it is far easier to take the first alternative, concentrate on those variables in other approaches that economists tend to use themselves, and do so in a way that meets economists' notions of consistency. However, my gut feeling is that by forcing other approaches into the economists' box, we lose the strength of the other approaches that we sought in the first place.

We gain a great deal from approaching institutions, events, and behavior as students of institutional economics, history, political science, and public choice. Such an approach helps us understand better how to define policy problems as well as why government acts and when and why people respond to government. Alexis de Tocqueville's *Ancien Regime*, for example, contained heretical (but telling) comments on the impact of institutional structure upon the scope, process, and content of government policy after the French Revolution.[1] He showed how the inherited institutions of the centralized monarchy influenced the new French Republic to behave like its predecessor. Somewhat the same could be said more than 100 years later about the behavior of Russia under Lenin and Stalin following Czarist patterns of political control and economic development.

In contrast to the long-term influence of old institutions, something very different happened in France after the Second World War when effective policymaking occurred only with the creation of new institutions that broke the pattern of their predecessors. Students of comparative government followed the post-war metamorphosis of the French republics with fascination. As in the pre-war period, government after government would crash, unable to cope with national needs. DeGaulle waited in the wings until an institution was created with enough central power to allow the executive and the legislative branches to function effectively. DeGaulle's unwillingness to step into the void until institutions stabilized helped force the creation of such an institution. Effective institutions for decisionmaking and policy implementation were the major forces in determining what was possible and what happened in France, not economic influences as economists now measure them.

Economists cannot ask the right questions about the expected success or impacts of a policy unless they have some appreciation for the other forces at work. This view is stated superbly by Lionel Robbins in his

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[1]Literature cited in the text is listed in the Bibliography section at the end of this essay.

lecture on "Economics and Political Economy." Some of these forces were once within the ken of political economy but are no longer a part of modern economics. Broad macroeconomic policy analysis suffers as a result.

On a somewhat different level, some of the most helpful public choice analysis includes economic considerations, but economic paradigms are not necessarily the driving force of the analysis. A good example of this is the work of Robert Bish and Vincent and Eleanor Ostrom who come from the government and public administration tradition. They investigated numerous factors that affect the choice of public services and their delivery mechanisms. Their research in public administration led to fresh treatment on many issues of interest to economists—like the identification and analysis of noneconomic factors influencing the cost and quality of public services.

Insightful noneconomics approaches need not be wedged into the economics paradigm to be useful to economists. I am convinced that economists must review and learn from the other approaches, but it is not essential (and may be harmful) to force others' methods into the economist's mode.

Time Frame and Scope Limitations

Economists impose both time frame and perspective limitations on themselves. Current agricultural policy appears to be the product of 5-year flashbacks. Each of the recent farm bills tries to deal with the problems of the previous 5 years. When the environment cooperates, the bill works moderately well. When there is a major change in conditions, Congress has to act again. The process is one of tinkering with existing mechanisms without much discussion of new societal goals or the possibility of a changing environment.

Agricultural policy instruments were mostly forged in the 1930's in response to the cataclysmic events of the Depression. The events of the 1920's and 1930's led to a willingness on the part of the New Dealers to try almost anything. If something did not work, it was jettisoned without ceremony, and something else was tried. Henry A. Wallace, the most politically powerful Secretary of Agriculture under President Franklin D. Roosevelt, ran a free-wheeling show that would have made the White House staff of any recent administration blanch. Little micromanagement of USDA came from the White House, and not much supervision flowed from the secretary's office to the rest of the Department. Apparently Roosevelt was willing to give a number of strong-willed people their head, reining them in only when complete disaster loomed. Wallace did some of the same during his 1933-40 tenure at USDA. This attitude was encouraged by a sense of political self-confidence that lasted until Democrats suffered severe losses in an interim congressional election. Opposition to change within government was confined to the small entrenched bureaucracy originally in place. The larger, newly created bureaucracies and institutions initially owed their existence and growth to promoting and implementing change. The policies possible under such circumstances differ radically from policy possibilities today. There is not the national sense of urgency about today's major national fiscal and financial problems that would be necessary to overcome the resistance of today's entrenched bureaucracy. This inaction also limits scope.

Ones Who Looked Forward

During the 1930's, economists with a broad view of agricultural policy were able to create their own program options, anticipating with uncanny accuracy the situations we face today. It is a unique experience for an economist today to read the *1940 Yearbook of Agriculture*, which was the social science yearbook of the Wallace era. Chester Davis's piece on the development of agricultural policy since the end of the First World War sets the stage like an operatic overture. He reminds us that "a nation's agricultural policy is not set forth in a single law, or even in a system of laws dealing with current farm problems. It is expressed in a complexity of laws and attitudes which, in the importance of their influence on agriculture, shade off from direct measures like the Agricultural Adjustment Act through the almost infinite fields of taxation, tariffs, international trade, and labor, money, credit and banking policy" (p. 325). Davis's broad and wise perception was not well remembered, to our cost, in the 1970's with respect to trade, and in the 1980's with respect to money, credit, and banking policy. Davis's candor on issues like the usefulness of parity price or parity incomes as measures of appropriate levels of income support is remarkable. He wonders "whether the objectives of agricultural policy can be once and for all established by a simple exercise in arithmetic" (p. 320).

Howard Tolley's piece in the same *Yearbook*, "Some Essentials of a Good Agricultural Policy," is both prescriptive and prophetic. He asks what farm people want in terms of a good life and defines these wants and their sources. Then he tries to determine policies that would best meet those wants, while taking into account the broad pattern of policy shaped by these wants during the previous decade. Tolley saw the objectives of farm policy during his era to be of three general types:

- "Activities designed to increase incomes of farmers who produce commodities for sale on a commercial scale;
- The efforts to raise incomes and to improve the living conditions of migrant laborers, sharecroppers, subsistence farmers, victims of drought or flood, and others at a disadvantage

within agriculture itself; and
- Activities designed to encourage better land use and more efficient production."

Tolley wrote that "most government programs of both the distant and the recent past have been directed toward improvement in the condition of commercial agriculture. It appears now [1940] that the last two of the groups of activities just listed will receive increasing attention in the immediate future" (p. 1,169).

The remarkable thing about agricultural policymakers in the 1930's was that they were not marginalists in their thinking. The severe nature of the problems confronting them and the inability of existing institutions and policies to cope with these problems encouraged an intellectual clean slate with a willingness (at times, propensity) to develop new approaches and new institutions. Some of Rex Tugwell's social experiments with the Resettlement Administration would be considered radical even today. (Tugwell was staff economist undersecretary to Wallace and head of the Resettlement Administration under Franklin Roosevelt.) The change that swept through the U.S. Department of Agriculture was such that many activists and visionaries believed that USDA was where the coming social revolution in America would originate.

Looking years ahead and recognizing the importance of resource conservation, Tolley suggested tying program participation to requiring farmers to "follow a system of farming that will more fully conserve the soil or control erosion than does their present system" (p. 1,175). He also recognized the continuing exodus of farm people to urban areas, its impacts, and its potential policy requirements. These architects of new programs did not see mere tinkering at the margin as sufficient to meet present needs, let alone the future goals of farm and urban people regarding agriculture and public welfare.

Marginalists in Objectives and Policies

A critical question today is whether policy economists are now marginalists, by natural selection if not by training. Is the discipline of economics, as we practice it and teach it, largely marginalist? It is not necessarily a bad thing to be a good marginalist. But, at times, our view of what is possible or appropriate for analysis or policy must be broader. For some past generations of economists, the paradigm, the profession, and the political system allowed them to be more than marginalists, especially in times of crisis.

Many persons are concerned about the paucity of new ideas from policy economists. These observers are asking us to cast a wider net in terms of both goals and policies for agriculture, natural resources, and the public welfare. Current agricultural and rural policies appear to have reached the end of the line in terms of public support, available finance, favorable international terms of trade, and a politically supportable agricultural structure and income distribution. It is not just fate that has caused agricultural program expenditures to be cut 25 percent in the budget compromise of 1990, a much greater proportional hit than any other major program.

Are we looking at continuing marginal changes in agricultural policy and agricultural policy mechanisms, or are we thinking ahead for basic structural changes in both policy and policy mechanisms? Mechanisms aside, how broadly is agricultural policy viewed? Should agricultural policy support *all* farmers regardless of their size and income levels, their environmental and conservation practices, their use of resources (especially subsidized resources like water), or their treatment of migrant labor? Are we continuing to craft farm programs to meet the first goal mentioned by Tolley, "activities designed to increase incomes of farmers who produce commodities for sale on a commercial scale," to the point where this objective by itself is no longer well supported by the general public? Are policy economists just marginalists looking narrowly at single-sectional goals rather than at the breadth of society's goals?

Tolley believed in 1940 that the first goal for agriculture had been sufficiently dealt with. We attempted to address the second goal in the Great Society programs and recently addressed the environmental and conservation concerns in the context of commercial farming policy. At what point should policy economists initiate a broader debate on goals for agriculture and rural people? Fresh and innovative thinking about where we go from here will have to go beyond current policies and their mechanisms and beyond marginal analysis. Marginalists survive well during stability. We are beyond stability, and instability demands something else.

Preparing for an Unstable World

What do those charged with agricultural policy do if the GATT negotiations continue to founder upon agricultural issues, trade wars break out, the United States has a prolonged recession, deficit and financial institutions continue to deteriorate, problems intensify, we continue to sell off productive assets to foreign firms, and all this leads people to believe that economic nationalism is the best approach to deal with our declining world economic power? Is commercial agricultural policy all we really need for agriculture and rural America? Do we finally pay real attention to the broader national policies Davis recognized as being fundamental to agriculture? Is anyone worried about this? Who has been thinking about more than marginal changes in existing policy and institutions to deal with such events?

Policy economists need to do good economics. They also need to be aware of what is going on beyond the economics paradigm. We cannot afford to be methodologically, intellectually, or politically bound in any way that narrows our approach to problem definition or problem solving. Today, the confines of institutional environment, training, intellectual scope, and methodology inhibit economists from exploring a sufficiently wide range of alternative policies and their consequences.

An interest in institutions and their influence, the study of individual and group behavior, and a knowledge of history force an economist to come to grips with things that go beyond marginal analysis. Recent world political and military events have been cataclysmic, not marginalist. This says that robust methodological approaches in economics that can reflect basic structural changes are more important than superior analytical performance that assumes stability. In addition, if we consider noneconomic investigations of human actions only to squeeze them into the economics paradigm, we will forfeit that very breadth and scope economics needs. An increasingly volatile world will require better understanding of fundamental human and institutional behavior. Investigations beyond economics must be used to broaden our perspective and enhance our view beyond the economics paradigm.

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A Career of Conceptualizing and Quantifying in Social Science

Karl A. Fox

This is the 30th anniversary of the establishment of the Economic Research Service, the 40th of my first article in *Agricultural Economics Research*, and the 50th of my enrollment in the graduate seminar which led me into agricultural economics.

Gene Wunderlich, the editor of this Journal, suggested that I address the role of agricultural economists in social science and the profession of knowledge building generally. I have been living this role with as much intensity as I could muster for almost 50 years. I will follow Gene's suggestion in terms of my own experience and leave most generalizations for my concluding remarks.

During 1942-54, I was a full-time member of the agricultural economics community. In it I found warm friendships, superb role models, and colleagues keenly interested in processes and institutions, open to useful ideas from several disciplines.

I am not sure that any agricultural economist is "typical." So, my path through the profession should not be expected to represent the profession as a whole. Nevertheless, a "case study" does put real flesh on the abstract body of ideas called economics and social science. It also shows that the needs and opportunities for particular contributions to knowledge depend on the data systems, methods, and bodies of theory available at a given time and on current and anticipated conditions in the economy and society. The contributions of an individual depend on knowledge acquired before entering the profession and knowledge gained during its practice.

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I acquired a superficial knowledge of the social sciences as a teenager, mainly because my father was a charter subscriber to the old *Encyclopaedia of the Social Sciences* (1930-35). I would leaf through each volume when it reached our home; the 15 volumes appeared at the rate of one every 4 months. I assumed then, as today, that all social sciences were equally relevant to our understanding of people in society.

Schooled in the traditions of several social sciences at the University of Utah, I completed an M.A. in sociology in 1938, then moved into general economics and completed my Ph.D. coursework and prelims at the University of California--Berkeley in that field. The most challenging job available when I was ready to take one was as an agricultural economist in the War Food Administration in San Francisco in 1942.

My career developed in the reverse sequence: 12 years in agricultural economics, 18 years largely in general economics, and 19 years working across the social sciences (including economics and agricultural economics). In all three phases, I emphasized visible, tangible, and measurable basic units and the integration of theory, methods, and data, traits I associate with agricultural economists.

A few influences accompanied me to my first job. At Berkeley, in 1941, I had studied quantitative agricultural price analysis with George Kuznets and Harry Wellman and advanced economic statistics with A.H. Mowbray. The principal text in both courses was Mordecai Ezekiel's *Methods of Correlation Analysis* (1930), which was the pre-eminent book on applied regression analysis.[1/]

Mowbray's course led me to Sewall Wright's 1921 article on "Correlation and Causation," which used arrow diagrams to state hypotheses concerning the causal relations connecting the

[1/] Sources are cited in the Bibliography section at the end of this article.

various members of a system of variables. I followed Wright's example in my diagrams of the demand-supply-price structures for farm products, which figured prominently in my later work on demand analysis.

In Robert A. Gordon's seminar on business cycle analysis, I had given reports on Jan Tinbergen's *Statistical Testing of Business Cycle Theories* (1939), and Henry Schultz's *The Theory and Measurement of Demand* (1938), works that established the frontiers of macroeconometrics and microeconometrics for the next 15 years.

These courses and works placed heavy emphasis on publications. Schultz, Tinbergen, and Wright were reporting their own original research. Ezekiel was one of the most productive research workers in agricultural economics in the 1920's, and his book reflected this. In effect, all four were teaching by example from the initial conceptualization through the interpretation of results.

A final influence was a good background in mathematics according to the standards of the time, including advanced calculus and advanced differential equations as an undergraduate and mathematical theory of probability as a graduate student. With a little effort, I could figure out whatever demonstrations in economic or statistical theory seemed relevant to my applied work.

That first job in San Francisco was an exercise in applied common sense. Asked for analyses relevant to support and ceiling prices for many fruit and vegetable crops, my supervisor, Cruz Venstrom, and I launched a fast-moving but comprehensive project that: studied the apparent responses of acreages to prices for each crop in each county; examined research reports on farm enterprise costs and yields; checked with California State statisticians to be sure we were interpreting their county average price series correctly; and surveyed experienced growers who were temporarily working for the regional office.

In each case, we selected a set of variables which we thought had accounted for most of the changes in acreage planted to a particular crop in a particular county. If two crops competed for land in a given county, their acreages were jointly determined. Our methods were informal, but we gradually built up a system of prices for many crops that we thought would have maintained their acreages at nearly constant levels during 1935-39. They provided the starting points for approximate adjustments to the cost levels projected for 1943, enabling us to handle requests for advice on different crops in a consistent manner.

In August 1944, I transferred to the regional office of USDA's Bureau of Agricultural Economics (BAE) in Berkeley to work 6 months with Marion Clawson, one of the best rough-and-ready economic analysts I ever met and a forthright and vigorous leader. Clawson had directed studies of the probable economic impacts of the Columbia Basin hydroelectric power and irrigation project in the Pacific Northwest and was coordinating similar studies of the Central Valley Project in California.

My first task was to draft a report on the prospective impacts of the Central Valley Project on industrial locations in California. Fortunately, the National Resources Planning Board had just published an outstanding report, *Industrial Location and National Resources* (1942), a good introduction to the field later known as regional economics. Mostly, I applied P. Sargant Florence's concepts of resource-oriented, consumer-oriented, and footloose industries to California data. Concepts of the economic base (including agriculture, which would expand with increased supplies of irrigation water) and an employment multiplier were at least implicit in Florence's approach and my application. Abundant supplies of electric power at projected low prices were also considered as a factor in attracting new industries.

Next stop was the Washington headquarters of BAE and work with its Chief, the brilliant and incisive O.V. Wells. Not long ago I heard Harold Breimyer refer to him as "the man with the trip-hammer mind." Breimyer had known him since the 1930's and regarded him as the principal (though anonymous) author of the Soil Conservation and Domestic Allotment Act, which salvaged and redirected the farm price support program after the original Agricultural Adjustment Act had been overturned by the Supreme Court.

During 1945-50, Wells used me on a series of urgent special assignments, each of which required me to collate and interpret forecasts of demands

and supplies for farm products comprising most of the agricultural sector. The specific forecasts were usually made by commodity specialists in BAE's Division of Statistical and Historical Research (S and HR), but as time went on I examined most of the formal and informal analyses on which their forecasts were based. The analyses were of varying quality, but there were no resources for improving the weaker ones.

The special assignments included 3 months (April-June 1945) as economist for the House Special Committee to Investigate Food Shortages, which held hearings and issued reports on the demand, supply, price control, and rationing situations for sugar, food fats and oils, and meat, poultry, and fish. Black markets were widespread and marketing channels distorted. It was a rare opportunity to observe demand and supply systems under pathological stress.

When the Marshall Plan for European economic recovery and development was proposed in June 1947, I assembled USDA estimates of the U.S. capacity to export specified agricultural products in support of the plan. As secretary of the Food Resources Subcommittee of the President's Committee on Foreign Aid, I worked with other secretaries to appraise the capacities of all relevant sectors of the U.S. economy and to prepare materials for the committee's consideration.

Also during 1947-48, I coordinated and edited a USDA report to the House Agriculture Committee on long-range prospects for American agriculture. During 1949-50, I helped coordinate an internal USDA study of the probable costs and consequences of alternative price support programs over a period of years under three different economic scenarios; the study was left incomplete when USDA's concerns shifted to shortages and inflation rather than surpluses and price supports in June 1950 with the outbreak of hostilities in Korea.

In the fall of 1950, I took stock of my professional situation. For several years I had been busy and happy with the special assignments which brought me approval and promotions from Wells and prestige among my colleagues in BAE. But I had published only one journal article and I had not submitted a dissertation to Berkeley.

I thought I could make an important contribution to demand analysis for farm and food products. I had internalized an enormous amount of information about the food and agricultural sector, read a large proportion of the published literature on demand analysis, and acquired up-to-date training in economic theory and econometrics.

The last coordinated set of demand studies by BAE economists had been published in the 1920's and early 1930's. Since then, BAE's data systems on food consumption and farm-to-retail price spreads had been greatly improved, as had the Commerce Department's data on disposable personal income. Some problems associated with multiple regression analysis of time series (multicollinearity, autocorrelation, the interpretation of significance tests) had been substantially clarified. And, there were now 20 years of data (1922-41) undisturbed by the special circumstances of World Wars I and II and their immediate aftermaths.

A consistent set of demand analyses for many farm and food products would be of value to S and HR commodity specialists in their economic outlook work and in their responses to requests for sectorwide projections and price-support program analyses, which were bound to recur. It might also be of interest to the profession as a whole.

During November-March 1950-51, I worked directly with S and HR's excellent commodity clerks and outstanding Computing Pool. The first result was an article, "Factors Affecting Farm Income, Farm Prices and Food Consumption" (1951a), which included about 70 statistically estimated equations. (Other Karl Fox publications are listed in "A Career in Print" at the end of this essay.)

The second result was a dissertation submitted to Berkeley in 1952, "The Demand for Farm Products," of which a condensed version was published by USDA as *The Analysis of Demand for Farm Products* (1953a). Both included arrow diagrams of the demand and price structures for major livestock products and for five categories of crops. The identification problem posed by Haavelmo in 1943-44 was carefully considered. I demonstrated that the demand functions for many

foods and farm products could be estimated without bias by ordinary least squares. I also showed that the demand functions for other commodities had to be estimated by simultaneous equation methods, for which I had neither time nor computing resources. These demonstrations formed the basis for my article on "Structural Analysis and the Measurement of Demand for Farm Products" (1954b).

From 1951 on, I carried in my mind a sort of econometric map of the agricultural economy, based on my arrow diagrams, empirical estimates of many of their components, and estimates of others. I could easily visualize elaborations of this national map to include demand and supply curves for specific commodities in each member of a set of regions. The existence of interregional trade in a given commodity would imply that prices in shipping and receiving regions would differ because of transportation costs. Quantitative models of interregional trade might permit S and HR specialists to regionalize their outlook statements for certain commodities.

I explored this idea in "A Spatial Equilibrium Model of the Livestock-Feed Economy in the United States" (1953b). The model involved a demand function for feed and a predetermined supply of feed in each of 10 regions, plus a matrix of transport costs between regions. It proved easy to solve and explain without using the formal methods and jargon of quadratic programming, and the model revealed important insights into a system accounting for more than half of all cash receipts from marketings of farm products. "The Use of Economic Models in Appraising Foreign Trade Policies" (1954a) adapted the 10-region model to explore the effects of general and discriminatory tariffs, subsidies, and changes in transport costs on prices, consumption, and trade in a hypothetical 10-country world. In this context, the spatial equilibrium approach also clarified some important issues.

In 1951, I decided to put on record in "The Measurement of Price Support Costs" (1951b) the logical and mathematical framework of the 1949-50 study of price support programs, previously mentioned. A May 1954 conference on Policies to Combat Depression led to my semi-empirical paper on "The Contribution of Farm Price Support Programs to General Economic Stability" (1956a), which followed the outlines of the 1949-50 study and also provided for agricultural feedback to the nonfarm economy. In effect, I combined a detailed model of the agricultural sector with a highly aggregated model of the nonfarm economy.

In 1955, Klein and Goldberger published a 20-equation model of the United States. In reviewing it in "Econometric Models of the United States" (1956b) I pointed out that the model's chief limitation was its high level of aggregation, much too high to accommodate significant information about particular sectors of the economy, such as agriculture, and to tap the knowledge and judgments of sector specialists. I observed that "our knowledge of the agricultural sector is adequate to support an econometric model of considerable complexity," including demand and supply functions for 20 to 25 major commodities of groups, and that such a model would be highly useful for policy purposes. A truly large-scale dynamic model of the U.S. economy would permit the advance appraisal of any set of economic policies and programs in relation to any initial positions and trends of the various sectors of the economy.

I never left the BAE, the BAE left me. Pieces of the former BAE were absorbed into new agencies in 1953-54. My position, absorbed into the Agricultural Marketing Service, involved a promotion and more administration than I cared for, so I joined the staff of the Council of Economic Advisers in 1954 and then moved to Iowa State University in 1955.

At Iowa State, research on commercial agriculture had been going well without my help. However, I found that a good many people in Iowa were concerned about the decline of small towns and the weakening of rural institutions. Some agricultural economists recognized that the seemingly inexorable process of farm enlargement was reducing the farm population and hurting small towns, but they were not paid to think about such things. State extension economists and sociologists handled individual requests originating in one or another of Iowa's 99 counties. The larger picture was ignored. Who would be foolish enough to produce an answer for which there was no question?

During 1955-60, pressure from concerned Iowans on university administrators brought more focused attention to problems of "agricultural adjustment." In 1961, the Kennedy administration responded to similar pressures at the national level and provided funds to the land-grant universities for programs of rural development which could involve areas larger than counties. Extension Service administrators at Iowa State asked me to suggest criteria for delineating such areas.

Studies by rural sociologists before World War I indicated that the residents of a small town and its trade area formed a relatively self-contained community. The boundary of the trade area was about five road-miles from the town or an hour's travel time with horse-drawn vehicles. The automobile had changed the face of rural America. When workers in Iowa began daily commutes in 1961, most of them headed for central business and industrial districts of Iowa's larger cities.

My research showed that a relatively self-contained community could be delineated around 11 or 12 Iowa cities, each of which was a wholesale trade center with a population of 25,000 or more. Each such city had a large number of jobs in a wide range of occupations and was the center of a labor market area, a home-to-work commuting area with a radius of about 50 road-miles and extending over several counties. I later called such areas FEA's, functional economic areas.

The trade and attendance areas for retail stores, schools, churches, and other voluntary associations were usually much smaller than an FEA. Most of the wages and salaries paid out by employers in an FEA would be spent at establishments within its boundaries, making it relatively easy to create FEA income and product accounts compatible with national economic accounts. The resident population of an FEA would also spend nearly all of its time within its boundaries during a given year.

The FEA delineations were used for several purposes in Iowa and the concept was rapidly disseminated across the United States. The Census Bureau commissioned a study by Brian J.L. Berry at the University of Chicago which delineated 358 FEA's, containing 96 percent of the U.S. population.

The FEA concept was endorsed by a committee of the Social Science Research Council on (Geographic) Areas for Social and Economic Statistics in 1967. A national system of FEA's comprised of clusters of contiguous whole counties could accommodate the county data bases of several agencies; the Standard Metropolitan Statistical Areas (SMSA's), which contained the central cities of the more populous FEA's; the additional counties within commuting distance of each SMSA; and the many nonmetropolitan FEA's centered on cities of less than 50,000 people. The Commerce Department's Bureau of Economic Analysis used the FEA's as a starting point for its system of BEA Economic Areas, which was used for regional economic projections by several U.S. agencies and by the National Planning Association.

During my tenure as a department head at Iowa State, I was fortunate to recruit some outstanding young economists. Two of them, Erik Thorbecke and Jati Sengupta, shared my admiration for Jan Tinbergen and were particularly enthusiastic about his 1952 book, *On the Theory of Economic Policy*. According to Tinbergen, a policymaker should classify the variables in a national econometric model as (1) targets, (2) instruments, (3) noncontrollable, and (4) irrelevant in relation to the array of policies under consideration at a particular time.

Desired values of the target variables in the coming year (for example, employment, income, and balance of payments) might be chosen intuitively, but in principle they (and some or all of the instruments) could be included in an objective function reflecting the policymaker's value system. Tinbergen emphasized the implications of a specified set of target values however chosen. Henri Theil, in his *Economic Forecasts and Policy* (1958), worked out the formal implications of an objective function. Theil's version of the "steering problem" had strong affinities with stochastic control theory.

Sengupta was well versed in quantitative economics and operations research, including stochastic control theory; Thorbecke was specializing in economic development. I joined them in coauthoring a book, *The Theory of Quantitative Economic Policy* (1966), which made major

extensions and applications of the Tinbergen and Theil approaches to economic growth and stabilization models and regional and sectoral analyses.

I extended my earlier conceptualizations in "The Study of Interactions Between Agriculture and the Nonfarm Economy--Local, Regional and National" (1962) and "Spatial Price Equilibrium and Process Analysis in the Food and Agricultural Sector" (1963). My most detailed conceptualization of the food and agricultural sectors of the United States and other large countries or multicountry regions was realized in 1969 in "Toward a Policy Model of World Economic Development with Special Attention to the Agricultural Sector." Regions classified according to soil and climate intersect with functional economic areas. Spatial equilibrium models are suggested both within and among 20 large world regions. In addition, concepts from several social sciences are introduced as relevant to the description and analysis of systems of villages, towns, and cities.

My work on FEA's had put me in touch with some outstanding urban and regional economists and quantitative geographers, and my membership on the Board of Directors of the Social Science Research Council (1963-67) involved semi-annual meetings with leading social scientists from several disciplines. These contacts stimulated me to read some of the best books published by social scientists in the 1950's and 1960's--the best disciplinary research, in Glenn Johnson's terminology.

The social indicators movement was launched in 1966 with impressive essays by outstanding scholars. Their immediate objective was to create a Council of Social Advisers (parallel to the Council of Economic Advisers), which would spearhead rapid development of data systems adequate for the guidance and evaluation of the many social programs initiated by the Johnson administration. A yearly *Social Report of the President* would appraise social conditions and recommend policy adjustments; a statistical appendix would present the relevant data.

This objective was not realized, but a demand for social indicators had been created and some highly eclectic work was published by people with little or no training in the social sciences. There was an enormous gap between social indicators and social theory, and I thought I could make an important contribution toward closing, or at least narrowing, it.

During 1972-73, a National Science Foundation (NSF) grant enabled me to spend 14 months on full-time research. By March 1973, I was ready to describe a framework which I thought would accommodate observable units and measurements from several social sciences, including economics. The framework would accommodate commercial agriculture and rural communities along with all other sectors and elements of an economy and society. The result: *Social Indicators and Social Theory: Elements of an Operational System* (1974), which advocated the supplementation, and eventual partial replacement, of social indicators by social accounts.

Elements of an operational system were designated at three levels: (1) individuals, families, and organizations in a small community; (2) cities and regions; and (3) national and world models and data. Subjectively, I felt I had written the book in the tradition of agricultural economics and dedicated it to Mordecai Ezekiel and Frederick V. Waugh, pioneers in combining measurement with theory.

While writing *Social Indicators and Social Theory*, I decided that "behavior settings," as defined by the psychologist Roger Barker of the University of Kansas, were promising basic units for a system of social accounts which would accommodate variables of interest to all of the social sciences. To implement such a system, we would need a criterion of comprehensiveness (what range of human activities should be included?); an objective method of classification that applied equally to market and nonmarket activities; and an objective unit for sampling and recording the contributions (inputs) people made to the social system and the rewards (outputs) they received from it.

I thought we could meet these requirements by viewing human societies from the perspective of "eco-behavioral science," a term introduced by Barker in 1969 after 22 years of trailblazing

research within a somewhat narrower framework which he called "ecological psychology"--the study of individual behavior in the settings of everyday life.

While observing children in a small Kansas town, Barker noted that their behaviors changed abruptly when they moved from one setting to another; these settings were units of the children's environment. Third-grade academic subjects called for one pattern of behavior, hallways for another, and lunchrooms for a third. Adults changed their behaviors abruptly as they moved from offices onto the town's streets and sidewalks and into restaurants, barbershops, or grocery stores. He concluded that the town, as an environment for human behavior, was *de facto* partitioned into hundreds of distinct observable units which he called behavior settings.

In various contexts, Barker and his colleagues asserted that "a school is its behavior settings" or "a community is its behavior settings." One colleague, Paul Gump, stated: "People live out their lives in a series of environmental units (behavior settings); experience in these settings is life. If experience is good, life expands; if it is bad, life diminishes." The study of behavior settings, and of organizations and communities viewed as systems of behavior settings, is called eco-behavioral science.

Barker and his associates made several comprehensive surveys of the behavior settings of a Kansas town. My 1990 book, *The Eco-Behavioral Approach to Surveys and Social Accounts for Rural Communities*, provides a detailed introduction to the method of behavior setting surveys and the microdata of Barker's last and most advanced survey. The book sets forth proposed applications and needs for multidisciplinary cooperation, highlighting the advantages of rural social scientists, including agricultural economists, in eco-behavioral research.

My 1985 book, *Social System Accounts: Linking Social and Economic Indicators through Tangible Behavior Settings*, and my 1989 article, "Behavior Settings and Social System Accounting," summarize my ideas for relating behavior settings to official data systems on establishments, industries, occupations, employment, and earnings; to the Standard industrial and Standard Occupational Classifications; to Commerce Department data on stocks of various types of physical capital and consumer durable goods; to studies of time use; and to the objective social indicators published since 1982 by the Organization for Economic Cooperation and Development (OECD).

Agricultural economists are engaged in a remarkably wide range of activities. Many of us have cooperated with applied scientists and engineers in problem solving and subject matter research. Neither we nor they have felt obliged to uphold disciplinary purity at all costs. I have seen cooperation in the same spirit between agricultural economists and rural sociologists.

I don't know how other agricultural economists feel about our relationship to "general" economics. If we are strictly in an area of application of economic theory, then perhaps we should not make room in our graduate programs for other kinds of theory. However, in our preface to *Systems Economics* (1987), Don Miles and I asserted that "the most promising frameworks for multidisciplinary cooperation can be expressed in the language of general systems theory, broadly conceived" (p. ix). We suggest that this book of essays by nine economists might be used as a guide for new and experimental courses in "systems economics" or "systems approaches to multidisciplinary research." Students may choose additional readings from references cited in the various essays.

I have emphasized social accounts for particular cross-sections of time since 1973. However, social accounts for successive years or quarters over a period of time would provide bases for dynamic models of macrosocial systems at national and regional levels. Some important conceptual work has been done toward dynamic models of microsocial systems based on behavior settings; these are annotated in pages 355-73 of *The Eco-Behavioral Approach to Surveys and Social Accounts for Rural Communities* (1990). Notable contributions include James R. Prescott's chapter, "A Behavior Setting Approach to Microanalytical Simulation Models at the Community Level," in *Social System Accounts* (1985) and a related

chapter by Prescott in *Systems Economics* (1987); Allan W. Wicker's article, "Behavior Settings Reconsidered: Temporal Stages, Resources, Internal Dynamics, Context" in *Handbook of Environmental Psychology* (1987); and Jati K. Sengupta's article, "Modeling Eco-Behavioral Systems," in *Mathematical Social Sciences*, Vol. 11 (1986).

My path through agricultural economics, economics, and social science has been a long one. In agricultural economics, my role models were Mordecai Ezekiel, O.V. Wells, and Fred Waugh; in economics, Jan Tinbergen; in quantitative methods, Sewall Wright and Herman Wold; in social science, Herbert Simon and Kenneth Boulding; in social accounting and model-building, Richard Stone; in eco-behavioral science, Roger Barker.

I felt a strong sense of community in agricultural economics during 1942-54, and I enjoyed warm relations among general economists during the 1960's. After 1971, my preoccupation with social system accounts and eco-behavioral science left me pretty much isolated from both communities, though not from particular colleagues who shared my new interests.

If I had remained continuously involved with agricultural economists, I might have realized that their concerns had broadened tremendously. My participation in a 1988 workshop sponsored by the Social Science Agricultural Agenda Project woke me up, but like Rip Van Winkle, I was not able to contribute much to the brave new world.

I have been greatly impressed by the accomplishments of my near-contemporaries--Glenn Johnson, James Bonnen, Harold Breimyer, and Vernon Ruttan, among others. The performance of some younger members of the profession marks them as worthy successors to Ezekiel, Wells, and Waugh. I believe the profession is well-equipped to generate new knowledge relevant to the enormously complicated economic, social, political, and environmental problems in which we are now immersed.

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